

PSYCHOLOGICAL REVIEW PUBLICATIONS

Psychological Review

EDITED BY

HERBERT S. LANGFELD
PRINCETON UNIVERSITY

CONTENTS

<i>A Study of the Continuity of the Problem-Solving Process: I. KRECHEVSKY</i>	107
<i>Recent Advances in Some Concepts of Conditioning: E. A. CULLER</i>	134
<i>A Critique of the 'Galvanic' Technique: CHRISTIAN A. RUCKMICK ..</i>	154
<i>A Reply to Professor Guthrie: EDWARD CHACE TOLMAN</i>	163
<i>An Eclectic View of Some Theories of Learning: W. N. KELLOGG</i>	165
<i>The Synthesis of Intelligence—Its Implications: THOMAS ROSS</i>	185

PUBLISHED BI-MONTHLY
FOR THE AMERICAN PSYCHOLOGICAL ASSOCIATION

BY THE
PSYCHOLOGICAL REVIEW COMPANY
PRINCE AND LEMON STS., LANCASTER, PA.
AND OHIO STATE UNIVERSITY, COLUMBUS, OHIO

Entered as second-class matter July 13, 1897, at the post-office at Lancaster, Pa., under
Act of Congress of March 3, 1879

PUBLICATIONS OF
THE AMERICAN PSYCHOLOGICAL ASSOCIATION

WILLARD L. VALENTINE, *Business Manager*

PSYCHOLOGICAL REVIEW

HERBERT S. LANGFELD, *Editor*
Princeton University

Contains original contributions only, appears bi-monthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 540 pages.

Subscription: \$5.50 (Foreign, \$5.75). Single copies, \$1.00.

PSYCHOLOGICAL BULLETIN

JOHN A. MCGROCH, *Editor*
Wesleyan University

Contains critical reviews of books and articles, psychological news and notes, university notices, and announcements. Appears monthly (10 issues), the annual volume comprising about 720 pages. Special issues of the BULLETIN consist of general reviews of recent work in some department of psychology.

Subscription: \$6.00 (Foreign, \$6.25). Single copies, 60c.

JOURNAL OF EXPERIMENTAL PSYCHOLOGY

S. W. FERNBERGER, *Editor*
University of Pennsylvania

Contains original contributions of an experimental character. Appears monthly (since January, 1937), two volumes per year, each volume of six numbers containing about 625 pages.

Subscription: \$14.00 (\$7.00 per volume; Foreign, \$7.25). Single copies, \$1.25.

PSYCHOLOGICAL ABSTRACTS

WALTER S. HUNTER, *Editor*
Brown University

Appears monthly, the twelve numbers and an index supplement making a volume of about 700 pages. The journal is devoted to the publication of non-critical abstracts of the world's literature in psychology and closely related subjects.

Subscription: \$7.00 (Foreign, \$7.25). Single copies, 75c.

PSYCHOLOGICAL MONOGRAPHS

JOHN F. DASHIELL, *Editor*
University of North Carolina

Consist of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. The price of single numbers varies according to their size. The MONOGRAPHS appear at irregular intervals and are gathered into volumes of about 500 pages.

Subscription: \$6.00 per volume (Foreign, \$6.30).

JOURNAL OF ABNORMAL AND SOCIAL PSYCHOLOGY

GORDON W. ALLPORT, *Editor*
Harvard University

Appears quarterly, January, April, July, October, the four numbers comprising a volume of 560 pages. The journal contains original contributions in the field of abnormal and social psychology, reviews, notes and news.

Subscription: \$5.00 (Foreign, \$5.25). Single copies, \$1.50.

COMBINATION RATES

Review and Bulletin: \$10.00 (Foreign, \$10.50).

Review and J. Exp. (2 vols.): \$17.00 (Foreign, \$17.75).

Bulletin and J. Exp. (2 vols.): \$18.00 (Foreign, \$18.75).

Review, Bulletin, and J. Exp. (2 vols.): \$22.00 (Foreign, \$23.00).

Subscriptions, orders, and business communications should be sent to

THE PSYCHOLOGICAL REVIEW COMPANY

THE OHIO STATE UNIVERSITY, COLUMBUS, OHIO

THE PSYCHOLOGICAL REVIEW

A STUDY OF THE CONTINUITY OF THE PROBLEM-SOLVING PROCESS

BY I. KRECHEVSKY

*Swarthmore College*¹

INTRODUCTION

As a result of recent experimental and theoretical work on the nature of the learning process a great deal of interest has been centered on the nature of the pre-solution period in discrimination learning. Lashley (10, p. 135) is perhaps responsible for the suggestion that the pre-solution period in discrimination learning needs special and distinct consideration apart from the rest of the learning process. He has suggested that

There are many indications that . . . in the discrimination box, responses to position, to alternation, or to cues from the experimenter's movements usually precede the reaction to light and represent attempted solutions that are within the rat's customary range of activity. . . . The form of the learning curve is the more significant when considered in relation to such behavior. . . . It suggests that the actual association is formed very quickly and that both the practice preceding and the errors following are irrelevant to the actual formation of the association.

The present writer, following this suggestion, has reported a number of experiments (5, 6, 7, 8) in which, by an appropriate analysis of the learning records of the individual animals, he has shown that the period preceding the actual learning or mastery of the required association did indeed consist of a series of 'attempted solutions,' and that during

¹ The experimental work reported in this paper was carried out in the Psychological Laboratory at the University of Chicago.

this period the animal systematically ran through a series of behavior patterns during which his responses were selective with respect to the possible stimuli present in any situation. That is to say, at first the animal might respond only to certain spatial differentia existing between the two choices offered him, then changing from that form of response, the animal might 'pay attention to' or react to certain other differentia until the animal finally came to respond to the 'important' differentia and quickly proceed with the solution of the problem set for him by the experimenter. These systematic behavior patterns were named by the present experimenter 'hypotheses.'

One possible interpretation of this series of experiments is that the animal during the time he was responding with these 'wrong hypotheses,' was not learning anything concerning the discrimination *per se* in which the experimenter was interested and which was the essential discrimination for the solution of the problem. This, however, is not a necessary interpretation from the data, since as was shown in the second experiment of this series (6) the pre-solution period is not sharply delimited into several series of mutually exclusive hypotheses. Very frequently the animals seemed to be responding to more than one hypothesis at the same time.

These two kinds of responses, spatial and visual, overlap in many cases. For example . . . every time the animal departed from his dark-going habit, he did so only to enter an alley on the right side of the box. . . . In other words, it may be that *on any one day the animal may have been responding not on the basis of one habit but two habits, a visual and a spatial one* (6, 58).

This would mean that in some instances the animal might be reacting to an 'irrelevant' set of discriminanda as well as to the relevant set, and would thus be learning something about the final solution even while seeming to be carrying out an 'irrelevant' hypothesis.

However the following interpretation can be made from both Lashley's suggestion and the data from the series of experiments already referred to: Since, during the so-called

pre-solution period, the animal does spend some time responding to 'irrelevant' discriminanda, the residual neurological effect of a 'rewarded' response or a 'punished' response is not the same during this period as during the period the animal is 'paying attention' to the relevant discriminanda.

Such an interpretation has some very serious consequences for several basic assumptions implicit in nearly all reports upon sensory discrimination in the rat and for all the theoretical mechanisms and rational learning equations which have been lately advanced concerning the nature of discrimination learning.

McCulloch and Pratt (12, 271) have stated as the three fundamental assumptions which are constantly found in nearly all reports upon discrimination-learning in the rat,

The assumption . . . that some quantitative aspect of the animal's performance preceding the solution, usually represented in terms of trials, errors, or time, reflects directly the capacity of the animal to make the discrimination. The further assumption is involved that, at least for practical purposes, the units of measurement may be considered as comparable. And still another assumption is involved, namely, that *repeated trials work in a cumulative manner to produce a change in the animal which is necessary for discrimination.* (Italics our own.)

This last assumption is also basic to all the theoretical structures of discrimination learning and all the rational learning equations with which the present author is familiar. Thus Spence has presented an elaborate conditioned-response 'hypothetical picture or logically possible account of the nature of discrimination learning' in which the following assumptions are stated:

According to the theoretical principles proposed by the writer [Spence] . . . the excitatory tendency of the positive stimulus (i.e., the strength of this stimulus to arouse the response of approaching it) is increased *in strength in a cumulative manner with each re-inforcement* while that of the negative stimulus, under certain conditions at least, is weakened cumulatively with each failure of re-inforcement. This process continues until the difference in the

excitatory strengths of the two cues is sufficiently great to offset consistently any differences in strength that exist between other stimulus aspects of the situation which might be allied in their action with one or the other of the cue stimuli. It follows from this theory that, provided that the excitatory strengths of the stimuli are initially equal or approximately so, their relative strengths will directly depend on the relative number of reinforcements and frustrations (non-reinforcements) they may subsequently receive (16).

Thurstone (18), Gulliksen (1), Wiley (20) and Gulliksen and Wolffe (2) have also presented us with rational learning equations which fundamentally involve this assumption. Thus, following Thorndike's 'law of effect' (17) which assumes that the 'stamping-out' effect of each repetition and punishment of the wrong response is *constant* during the course of learning and correspondingly that the 'stamping-in' effect of each repetition and reward of the correct response is also constant, Gulliksen presents the following equation for the learning process:

$$u = \frac{g}{c} \left[1 - \left(\frac{\frac{h}{\bar{k}}}{w + \frac{h}{\bar{k}}} \right)^{\frac{c}{\bar{k}}} \right],$$

where u represents cumulative errors, w represents cumulative correct responses, g is the initial strength of the incorrect response, h is the initial strength of the correct response, \bar{k} is the constant amount added to the strength of the correct response each time it is repeated and rewarded, c is the constant amount subtracted from the strength of the incorrect response each time it is repeated and punished.

In a later presentation of a rational theory of discrimination learning Gulliksen and Wolffe (2) give as one of their basic assumptions of their newer formula "*Each reward increases the strength of a response tendency and each punishment decreases it*" (italics our own). Here also the increase or decrease in strength is postulated as a constant.

Similar examples could be cited from Thurstone and Wiley.

It can readily be seen that the assumptions listed by McCulloch and Pratt and the basic assumption found in Spence's, Thurstone's, Gulliksen's, and Wiley's theoretical formulations are directly contradicted by the assumption based on the interpretation suggested by Lashley and the present writer.

Simply restated the former assumption (which we shall from now on refer to as the 'continuity' assumption) says that once an animal is immersed in a given problem situation (more specifically, discrimination-box) *each time* the animal makes a choice an effect is recorded on his nervous system. This effect is specific and constant. Every time, from the very first trial on, the animal makes a 'correct' response the strength of the tendency to respond positively *to the specific stimulus the experimenter has in mind*, is strengthened; each time, also from the very beginning, he makes an 'incorrect' response, the strength of the tendency to respond positively to the specific stimulus the experimenter has in mind is weakened, and further, these increments (or decrements) are constant throughout the learning process. Nothing is said about the 'attention' of the animal, nothing about the 'awareness' of the animal with respect to the important stimuli. Thus stated, and not unfairly stated, this assumption sounds very much like the old principles of frequency and effect clothed in 'k's' and 'c's' and 'excitatory strengths.'

On the other hand, the latter assumption (which we shall from now on refer to as the 'non-continuity' assumption) just as simply stated, says that once an animal is immersed in a given problem-situation the animal selects out of the welter of possible stimuli certain sets of discriminanda to which he reacts. Each time (while 'paying attention to' this particular set of discriminanda) he makes what proves to be a 'correct' response, he learns (wrongly perhaps) something about the significance *of this particular stimulus*; each time he makes a 'wrong' response, he learns something else, *but he does not learn anything about the 'correctness' or 'wrongness' of the to-be-finally-learned set of discriminanda*. Eventually he gives up responding to his first set of discriminanda

and responds to another set, and another set, etc., until he begins to respond to the relevant set. From then on, and from then on only, is he learning anything about the discrimination involved, or, from then on only are his 'bonds' being strengthened, etc.²

A number of studies are already available, of course, which bear on the general question of the efficacy of frequency in 'stamping-in' a response. Thus the experiments of Peterson (13, 14), Higginson (3), Tolman and Honzik (19), Maier (11), Krechevsky and Honzik (9) and many others all seem to demonstrate that the frequency of a given response to a given stimulus situation does not initiate the learned response nor does it prevent this response from rapidly being dropped in favor of more appropriate responses to the same stimuli when the situation demands such a change. But these are only of general applicability to the question at issue here. The writer is familiar with but two experiments which have actually formulated the problem as stated here and attempted to obtain some relevant evidence for its solution. These are the studies of McCulloch and Pratt (12) and Spence (16). A more detailed discussion of these studies will be given later, after the results of this study have been presented.

The two assumptions under discussion are so stated as to permit of a crucial test. One of the corollaries of the 'non-continuity' assumption would be that if the significance of the

² This latter assumption and its corresponding description of the learning process has a great deal of similarity to the description of learning suggested by the Gestalt psychologists. They have consistently emphasized the necessity of including the state of the animal's perception in describing what is going on when an animal is attempting to solve a given problem in a given situation. The animal starts off, they point out, with some definite organization of the stimuli confronting him. The specific kind of 'ordering' of these stimuli in the animal's perception determines in a large measure just what the animal will react to. As the training procedure progresses, the animal must reorganize and reorganize his stimulus field until he achieves an order which permits of the final solution, and until this final reorganization has taken place, the animal is learning very little about the problem set for him by the experimenter—no matter how much he may be learning about other things. In a very brief descriptive summary of the Gestalt theory of learning, Koffka writes (4, p. 136), "To cut a long story short, we find at the beginning . . . in training and intelligent performances, unitary, articulate, meaningful wholes. . . . Development proceeds by transformation of such structures. Gradually, by a number of leaps and bounds we achieve different orders, different articulations, different meanings."

stimuli are reversed before the animal begins to 'pay attention to' them (*i.e.*, during the pre-solution period) it should not necessarily make for any slower learning of the reversed problem. That is to say, if, during the pre-solution period, the positive stimulus is made negative and the negative stimulus positive, and if, after this pre-solution period, the cues are reversed, these animals should make as good a score as a control group which started out with the 'correct' values of the cue stimuli from the beginning. On the other hand, assuming the 'continuity' assumption, we would definitely expect just the opposite set of results. In this latter case we would expect to find the group which started with the stimuli 'in reverse' to be much inferior to the control group, since from the very beginning (and therefore throughout the pre-solution period) the animals have been strengthening their tendency to make a positive response to what is later to be the negative stimulus.

Specifically, then, the purpose of the present experiment was to test the adequacy of the two assumptions in question by testing the resulting set of corollaries.

APPARATUS AND METHOD

Since most of the theoretical structures for the learning process which have been presented are based on visual discrimination learning in a situation where only two differing visual stimuli are presented (it being assumed that this is a 'simple' situation and therefore better adapted for theoretical manipulation) we used, as our problem for the animals, the discrimination between two such visual stimuli. The stimuli which were to be differentiated between are presented in Fig. 1. The apparatus used was the Lashley jumping-stand for visual discrimination training.

Forty-seven rats, divided into three groups, were used as the subjects in the present experiment. These animals were all males, approximately three months of age at the beginning of the experiment, and were taken from the regular supply of the Chicago Psychological Laboratory. After a seven-day preliminary training period during which the animals were

permitted to become adapted to the apparatus and during which they were trained to jump through blank cards, the three groups received the following training.

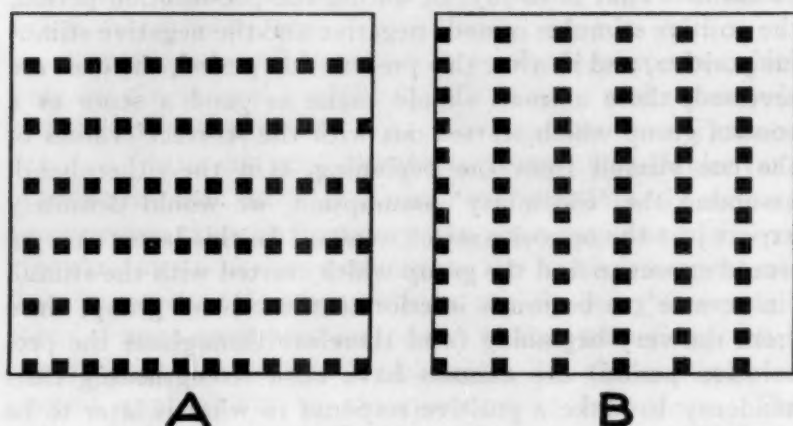


FIG. 1. Stimulus cards

Group I ($n = 17$).—This group served as the *Control Group*. Immediately after the preliminary training period referred to above they were started on the regular discrimination training in which stimulus 'A' was the positive stimulus, and 'B,' the negative stimulus (Fig. 1). The training schedule consisted of ten trials per day until each animal met the criterion set up as indicating complete mastery. To meet this criterion the animal was required to make at least eighteen correct choices on any two consecutive days, *i.e.*, 18 correct out of twenty, or 90 per cent correct.

The order of right-left presentations of the correct stimulus was the standard order used in the Chicago Laboratories for discrimination training. Each stimulus presentation was allowed to stand until the animal had made the correct response. That is to say, during the early part of the training period the animals very frequently would jump to the negative card, would be placed back on the starting stand, and then repeat the same error, and continue to repeat that error for a number of times. Each incorrect response was recorded as an 'error' so that on any one trial, one or more errors were possible. A fuller discussion of this scoring procedure will be

given later. Although each wrong jump was counted an error, the records were so kept as to enable us to fractionate the errors into 'repetitive errors' and 'initial errors.'

Group II ($n = 14$).—This group served as our *Two-Day Experimental Group*. After the preliminary training period these animals were presented with the two experimental stimulus cards, but with reverse significance, *i.e.*, card 'B' was the positive stimulus and card 'A,' the negative stimulus. This arrangement was continued for two days, or a total of twenty trials. On the third day, or the 21st trial, the arrangement was reversed so that from then on card 'A' was positive and card 'B' negative, as in the case of the Control Group. These animals were then continued with their training until they had met the criterion described above and had mastered the 'A-positive, B-negative' discrimination. The same scoring procedure was used for this group as for the Control Group.

Group III ($n = 16$).—This group served as the *Four-day Experimental Group*. As in the case of Group II, these animals were presented, initially, with card 'B' as positive and card 'A' as negative. This arrangement was continued for four days, or a total of forty trials. On the fifth day, or the forty-first trial, the arrangement was reversed and the animals continued until they had mastered the 'A-positive, B-negative' discrimination.

If, now, the continuity assumption is correct, we would expect that the total trial-and-error score made by Group I should be less than the total trial-and-error scores made by Groups II and III (counting the records of Groups II and III from the point of shift, *i.e.*, from day three on for Group II and day five on for Group III). This follows since according to this assumption, the strength of the 'bonds' between stimulus 'A' and the positive response to that stimulus, is *continuously and constantly increased* from the very first experience with it; on the other hand, for Group II just the opposite effect is taking place during the first two days, and for Group III, during the first four days. In these latter two cases, during the pre-shift period, not only is the 'bond'

between the stimulus 'A' and a positive response to it *not* being strengthened, *but the 'bond' between the stimulus 'B' and a positive response to it is being strengthened.* The result should be, therefore, that when the significance of the cards is reversed, the animals of the two experimental groups should start off with a decided handicap. They must first 'unlearn' all that has presumably been learned during the first period and then 'catch up' to the advanced stage in the learning process in which presumably the control animals are in at the twenty-first and forty-first trial respectively.

If the 'non-continuity' assumption is more nearly correct, then we should expect that Group II from the point of shift on (and perhaps also Group III) should make *less* errors and take *less* trials in mastering the discrimination than the total error-and-trial score made by the Control Group, and, further, that the error-and-trial score of Group II from the point of shift on should be approximately *equal* to the error-and-trial score made by the control animals *from the twenty-first trial on.* These expectations follow since according to the 'non-continuity' assumption the animal in the first two days is responding to the 'wrong' set of discriminanda and is therefore not profiting at all by his experience, as far as the final discrimination itself is concerned. It would make but little difference, therefore, whether the positive stimulus were 'A' or 'B,' the 'errors' accumulated by the control animal during this period are, as it were, 'wasted.' If, therefore, we begin crediting the animal of Group II with errors *after* this period, instead of from the very start, the animal's final total score would be smaller than the score of the animal whose errors from the very beginning were counted.

All this assumes, of course, that the animals of Group II had not already completed their 'preliminary hypotheses' and started on their final hypothesis within the first twenty trials. On the basis of the records of the control group (ten of which had been run first) we were led to believe that we were quite safe in assuming this.

In the case of the four-day experimental group (Group III) we might expect one of two sets of results: (1) If four days

were too long for the pre-solution period (*i.e.*, if within the first forty trials the animals were already *beginning* to react to the significant set of stimuli) then the score for this group might be slightly less, approximately equal, or slightly greater than the score for the control group, depending on how soon after the first twenty trials the animals of Group III began to react to the significant discriminanda. We would thus obtain some data on the length of the pre-solution period for this particular problem situation. (2) If four days were still within the pre-solution period, then we might expect that this group would not only earn a better score than the control group, but also a better score than Group II, since that would mean that we had begun to 'credit' the two-day group with errors too early.

In any event, the results to be expected on the basis of the 'continuity' assumption are distinctly different from the results to be expected on the basis of the alternative assumption of 'non-continuity.'

RESULTS AND DISCUSSION

The presentation of the results will be divided into a comparison of the Control Group with Group II, and the Control Group with Group III.

TABLE I

COMPLETE TRIAL-AND-ERROR SCORES FOR GROUPS I AND II

	Average Total Errors	Average Initial Errors	Average Repetitive Errors	Average Trials
Group I.....	99.50	55.23	44.35	162.35
Group II.....	61.80	40.40	21.40	120.71
Difference.....	37.70	14.83	22.95	41.64
<i>t</i>	3.099	2.371	2.944	2.631

Table I presents the data for the comparison of the performance of the Control Group with that of Group II (the 'Two-Day Group'). It will be seen that we have fractionated the error scores for both groups into 'initial' errors and 'repe-

titive' errors. An 'initial' error is defined as an incorrect response of the animal when he is first presented with any given stimulus arrangement, *i.e.*, the first error on any given trial. A 'repetitive' error is defined as any subsequent error made on the very same trial. Thus the maximum number of 'initial' errors an animal could make on any one day (ten trials) was ten, while there was, theoretically, no limit to the number of 'repetitive' errors the animal could make, since he was permitted to jump until he finally made the correct response before the trial was considered ended.³ The problem then presents itself as to how to count errors. Should the error score of the animal be considered the total of the 'initial' errors plus the 'repetitive' errors, or should we merely count the 'initial' errors as the animal's error score? The usual procedure with the jumping-stand technique is to use the sum of the two kinds of errors as the animal's score. This procedure seems to us to be adequate, but the reason we raise any question at all about it is that this procedure does introduce a complicating factor, since it appears that the jumping-stand apparatus is one which seems to favor the occurrence of repetitive errors, and these errors appear most frequently in the early part of the training procedure—the very period we are most interested in. A fuller discussion of this point will be made later, but for the present we might point out that for very sound theoretical reasons which follow from the 'continuity' assumption we *must* give equal weight to both initial and repetitive errors. It will be remembered that according to that assumption the *only variables considered* were those of 'response,' 'reward' and 'non-reward.' Cer-

³ This procedure, however, was not followed during the very first day of training (on the standard set-up for the Control Group and the converse set-up for Groups II and III). During this first day, after the animal had made five repetitive errors on any one trial, the positive card was removed from the window and the animal was allowed to jump through the open window to the food stand. This was done to speed up the learning process. This does not, however, prejudice the results in favor of the 'non-continuity' expectations, but, if anything, does just the opposite, since the only possible effect of this procedure is to reduce the number of errors made on the first day. This would, proportionally, increase the number of errors made on the other days, thus tending to decrease any differences between the total errors made by Group I and the errors made by Group II after the shift.

tainly a 'repetitive error' is as much a response as an initial error, and just as certainly the punishment following a repetitive error is as much non-reinforcement as the punishment from an initial error. No variables are found in that assumption which would distinguish between one kind of a positive response and another, one kind of reinforcement and another. Further, no provision is made in that assumption for increments or decrements of variable magnitudes, so that we cannot assume that the increment resulting from one response is larger than the increment resulting from another response, etc.

However, while the total error score is thus the significant one to consider in this discussion, we will present an analysis made on both bases, *i.e.*, counting both initial and repetitive errors together and separately.

Referring again to Table 1 we see that considering all the measures available the scores for Group II are *in every case* smaller than the scores for the Control Group. Where the Control Group made a total of 99.5 errors, Group II made a total of 61.8. (These are average error scores.) The difference between the averages, 37.7, represents a saving of 37.8 per cent for the Experimental Group over the Control. This means, it seems to us, that for the Control Group some 37 per cent of the 'error' responses with the attendant 'frustrations' or 'non-reinforcements' had no effect on the learning behavior of the rat as far as the final discrimination is concerned. The same results are apparent if we consider only the initial errors. From an average error score of 55.23 for the Control Group, the score drops to 40.40 for Group II. The differences between the scores are statistically reliable, the greatest probability that the least of the differences in the table is due to chance is less than .02.

These differences between the two groups are not only apparent from a consideration of the total scores earned, but also from the analysis of the daily performances of the animals. In Fig. 2 are presented the daily average error scores for the three groups. The curve for Group II starts on the third day. It will be noticed that the animals of Group II started off at

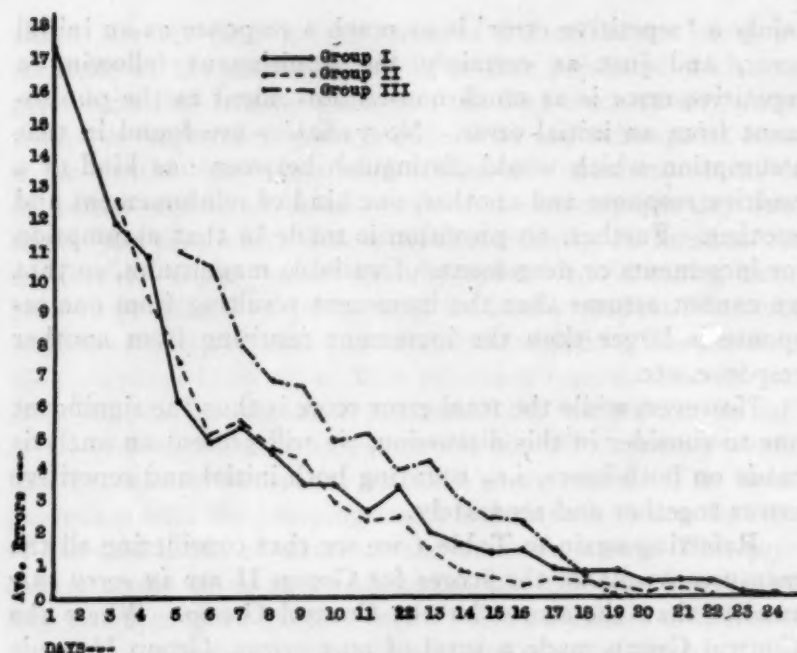


FIG. 2. Daily average error curves

almost the same point reached by the Control animals after two days experience with the discrimination training. From that point on the two curves show a very great degree of correspondence—almost to the point of identity.

Referring now to Table 2 for a comparison between the records of Group II with the truncated records of the Control Group (*i.e.*, from trial twenty-one on) we again find the expectations based on the 'non-continuity' assumption ful-

TABLE 2
TRIAL-AND-ERROR SCORES FOR GROUP I (TRUNCATED) AND GROUP II

	Average Total Errors	Average Initial Errors	Average Repetitive Errors	Average Trials
Group I.....	67.00	43.70	23.30	142.35
Group II.....	61.80	40.40	21.40	120.71
Difference.....	5.20	3.30	1.90	21.54
<i>t</i>468	.536	.281	1.343

filled. In this comparison, it will be remembered we should find, on the basis of our assumption, both the experimental and Control groups making approximately the same score, whereas, according to the 'continuity' assumption we should expect a noticeable difference in favor of the control group. An inspection of the data in Table 2 shows the existence of a clear similarity between the performances of the two groups. For any of the measures used the differences which do exist are small and statistically unreliable. This similarity in scores means, of course, that all the excess errors for the Control Group which were observed when the data in Table 1 were analyzed, resulted from counting as 'errors' the incorrect responses of the Control Group during the first two days of the pre-solution period.

TABLE 3
INCIDENCE OF SPATIAL HYPOTHESES—GROUP I

Day	1	2	3	4	5	6	7	8	9	10
A. S.....	0	0	1	1	0	0	0	0	1	2
P. S.....	6	2	1	0	0	0	1	1	0	0
L.....	0	0	0	2	0	0	0	0	0	0
R.....	0	0	0	0	0	1	0	0	0	0
Total.....	6	2	2	3	0	1	1	1	1	2

A. S. refers to Alternating Spatial hypotheses, P. S. to Perseverative Spatial, L. to Left and R. to Right hypothesis.

These results already, it seems to us, can not possibly be reconciled with the assumptions of Spence, Thurstone, Gulliksen, et al, relative to the continuity and constancy of the problem-solving process. There is no evidence whatsoever that the animals of the Control Group learned anything at all about the final discrimination in the first two days of the experiment. Further, it seems clear that these data also contradict the assumption that the total error score, as usually presented in reports on discrimination learning, represents directly the capacity of the animal to make the discrimination and that these 'errors' are comparable to the 'errors' made later in the learning process.

The data show clearly and unambiguously that the experimental group *had* been learning something of value during

the first two days of the experiment (their score being approximately 37 per cent better than it would have been had they not been given that training) but quite obviously that something had nothing to do with the specific responses to the specific stimuli involved in the final mastery of the problem. The most reasonable description of what went on during these two first days seems to us to be something like this: The animals of both Groups I and II learned (a) that certain spatial hypotheses were 'wrong' and after attempting them, eliminated them from their store of behavior possibilities; (b) they also had made a great deal of progress in learning to eliminate repetitive errors. The mastery of this latter problem is of great significance in this situation. That this did occur is demonstrated by the fact that fifty per cent of all the repetitive responses made by Group I were made during the first two days. The total repetitive score for Group I was 44.35 (Table 1), while the repetitive error score for the same group minus the performance on the first two days was 23.7 (Table 2). Having learned this simple, but important, fact about the experimental situation (plus whatever else may have been learned about the irrelevance of certain spatial hypotheses)⁴ the animals of the experimental group entered the first 'experimental' trial, *i.e.*, the twenty-first trial of training, with a decided advantage over the animals of the Control Group on their first 'experimental' trial, *i.e.*, the first trial of the first day of training.

It will be remembered that in order to get additional data on the limits of the pre-solution period, we ran a third group of animals (Group III), the Four-Day Experimental Group. Here again we should expect a poorer performance on the part of this group when compared with the Control Group if the 'continuity' assumption were correct. On the other hand, as was pointed out above, the expectations could not be very definite as far as the 'non-continuity' assumption is concerned. We could be more definite in our expectations if we

⁴That they learned about such irrelevancies is demonstrated by the fact that of the total of nineteen spatial hypotheses shown by the Control Group in the twenty-odd days of training, eight of them, or 42.4 per cent, occurred within the *first two days* (Table 3).

had some information as to the actual length of the pre-solution period for this particular visual discrimination problem with this apparatus. However, the fact remains, that if certain results *did* appear, the 'continuity' assumption could not be reconciled with them.

The data for the comparisons of this group with the Control Group are given in Tables 4 and 5. Referring first

TABLE 4

COMPLETE TRIAL-AND-ERROR SCORES FOR GROUPS I AND III

	Average Total Errors	Average Initial Errors	Average Repetitive Errors	Average Trials
Group I.....	99.50	55.23	44.35	162.35
Group III.....	70.50	47.00	23.31	131.21
Difference.....	29.00	8.23	21.04	31.14
<i>t</i>	2.624	1.418	3.073	2.203

TABLE 5

TRIAL-AND-ERROR SCORES FOR GROUP I (TRUNCATED) AND GROUP III

	Average Total Errors	Average Initial Errors	Average Repetitive Errors	Average Trials
Group I.....	44.94	33.82	11.11	122.35
Group III.....	70.50	47.00	23.31	131.21
Difference.....	25.56	13.18	12.19	8.86
<i>t</i>	2.927	2.349	2.874	.627

to Table 4 we see that again, using every measure available, the experimental group is superior to the Control Group. Where the Control Group made an average of 99.5 errors, the animals of Group III averaged 70.5 errors. The *t* of this difference, 2.624, indicates a probability of greater than .99 that this is a true difference. In terms of repetitive errors there exists a difference of 21.04 errors, or approximately a saving of 50 per cent for the experimental group over the error score of the Control Group. This difference is also highly reliable. In terms of trials, the animals of Group III

required 31.14 less trials than did the Control Group. The smallest difference is in terms of 'initial' errors. There the difference in favor of Group III is but an average of 8.23 with a relatively small t (1.418), indicating no great statistical significance. Thus, on the whole, *Group III, contrary to the expectations of the 'continuity' hypothesis, is superior to the Control Group.* However, the differences between the two groups are not as great as the differences noted above between the Control Group and Group II. This immediately suggests, in terms of our analysis, that somewhere after the 20th trial and before the forty-first trial the animals of Group III had already begun to react to the significant discriminanda with the consequence that a reversal of the cues after the fortieth trial resulted in some degree of negative transfer.

Considering now the data in Table 5—a comparison between the truncated record of the Control Group with Group III—we find for the very first time differences in favor of the Control Group. Some of these differences seem to be statistically significant. But such differences in favor of the Control Group are exactly what we would expect with our 'non-continuity' hypothesis if we assume that four days is too long a time for the purely pre-solution period. Of course the 'continuity' hypothesis would also predict differences in the same direction, but, perhaps, larger differences. Since both assumptions, then, would have predicted the same direction of differences which were found here, the data from this one comparison do not lead unambiguously to the verification of either assumption as being the more nearly correct one.

In general, considering all four comparisons made, it appears clear that (1) the first three sets of data unambiguously agree with the expectations of the 'non-continuity' assumption; the fourth set of data also agrees with this assumption if we make the further reasonable assumption that the pre-solution period occupies less than four training days (forty trials). (2) The first three sets of data, on the other hand, clearly contradict the predictions to be made on the basis of the 'continuity' hypothesis, while the fourth set of data may, or may not, agree with the prediction.

On the basis, therefore, of all the data and considerations presented up to this point, we feel justified in making the following conclusions: (1) The problem-solving behavior of an animal in visual discrimination training does not justify the assumption that "the excitatory tendency of the positive stimulus is increased in strength in a cumulative manner with each reinforcement while that of the negative stimulus is weakened cumulatively with each failure of reinforcement." (2) Neither is the assumption justified that some quantitative aspect of the animal's performance, trials, errors or time, reflects directly the capacity of the animal to make the discrimination. (3) Instead it appears that there exists a pre-solution period during which the animal is responding to 'irrelevant' discriminanda in the experimental situation and that (4) the residual effect of a rewarded response or a punished response is not the same during this period as during some later period in the learning process.

It was pointed out in the Introduction that two experiments already exist bearing directly on these questions. On the basis of these experiments those writers (McCulloch and Pratt, 12, and Spence, 16) concluded that the 'continuity' assumption is justified. We shall now consider their experimental work in relation to the apparently contradictory data we are reporting here.

The first experiment to be considered is that of Spence. In this experiment Spence attempts to support his theoretical position that one can admit the fact of the existence of organized, selective and systematic behavior during the pre-solution period (as presented by the present writer in the 'hypothesis' series of experiments already referred to) and still maintain the 'continuity' assumption. The proof of the validity of the 'continuity' assumption was the chief object of his experiment.

His procedure was to present his subjects (twelve adult chimpanzees of which eleven had been used in one or more previous psychological experiments) with

a series of discrimination problems involving four different stimulus forms. . . . Each animal was first

taught two preliminary discriminations: A (+) versus B and, after the completion of this, C (+) versus D. Following the *learning* (italics our own) of these preliminary problems the animals were presented with five tests consisting of five new learning problems in which the same stimuli in various combinations were used. The first of these tests involved . . . half of the animals having stimulus A positive (A versus C) and half stimulus C (C versus A). The second test . . . the subjects that had stimulus A positive in the first test problem having stimulus D made positive (D versus B) and those that had stimulus C positive in test I having stimulus B made positive (B versus D) . . . etc.

If our theory of discrimination learning is adequate there should be a definite relationship between the learning of each of these test problems . . . and the relative number of reinforcements and non-reinforcements the stimuli have had in previous problems (16, 78-79).

This general relationship Spence finds to hold true upon the completion of the experiment, and therefore concludes that "The results of this experiment are interpreted as supporting the theory that discrimination learning in animals is a cumulative process resulting from differential reinforcement and non-reinforcement of responses to the relevant stimulus components."

A careful analysis of his experimental conditions and the logic of his 'interpretation' leads the present writer, however, to suggest that the arrangement of the experiment of Spence did not permit the collection of data relevant to the problem investigated, and that therefore his 'interpretation' is a gratuitous one.

It will be noticed, from the description of his experimental procedure, that first he trained his animals *until they had mastered the discriminations between the two sets of stimuli which were later to be used in the 'test' trials*. This, in our terminology, means that he had trained the animals to 'pay attention to' or 'react to' the relevant discriminanda. Transferring the animals, after that point had been reached, to another set of problems involving the same stimuli, can not,

in our opinion, give us any data on what had gone on *during the pre-solution period*. There is no longer a pre-solution period for the 'test' problems in the sense that the animals might reasonably be expected to react to any other set of discriminanda, such as spatial differentia, etc. All that Spence has demonstrated with his experiment is that *after* the animals had learned to respond to one stimulus, and discriminate that from another, they were able to transfer that experience to another problem where the same stimulus was involved, and that the degree of transfer seemed to be a function of the difficulty of the original problem. What we really want to know, in attacking the present problem, is whether during the pre-solution period, *before* an extended training has forced the animal to react to the relevant discriminanda, the effect of reinforcing or failing to reinforce a response to the 'correct' stimulus has any cumulative effect. Therefore we cannot admit the data from Spence as contradicting our data.

The experiment of McCulloch and Pratt (12, 273), however, is set up so as to permit argument from the resulting data to the question attacked. The procedure they used was closely similar to the procedure used in the present experiment.

One part of this investigation consisted in a study of the effect upon later discrimination in a standard discrimination set-up [positive stimulus, food on a 75-gram weighted tray; negative stimulus, a 25-gram weighted empty tray] of first training the animals with the relevant stimuli conversely arranged for several (28) trials . . . *i.e.*, with the stimulus which was later to lead on to food now not leading on to food and the stimulus which was later not to lead on to food now leading on to food. It was considered that, if the first several trials constitute a familiarization period, these animals upon being shifted to a SD [Standard Discrimination] set-up should learn the problem more rapidly than should a control group not given such a preliminary training period. The whole training period, including the CSD [Converse Standard Discrimination] and the SD periods, might possibly be no longer than the training of the control group.

The discrimination involved, as has been pointed out, was one of weight discrimination, in which the animals were required to pull in one of two strings to which were attached trays of differing weights (75 and 25 grams), with one of the trays being 'loaded' with food.

It will be seen that this procedure of McCulloch and Pratt is similar to ours and that they list as one of the possible sets of results exactly what we have found from our experiment. However, in their experiment, just the opposite results were obtained.

A comparison of the records on the SD set-up of . . . the control group, with that on the same set-up of Group I, which was first trained for twenty-eight trials with food on the lighter tray, furnishes evidence as to the nature of the early training period. . . . Even after the shift, Group II made, on the average, 12.31 (24 per cent) more errors than did the control group. . . . The probability that there is a true difference is .9979 (12, 279-280).

They therefore conclude that "there is a cumulative effect of training and that this effect is roughly proportional to the number of errors made."

Apparently there seems to be a direct contradiction between their set of results and our results. There are, however, a number of factors which might account for this apparent contradiction without negating the interpretation we have made on the basis of our own data.

(1) The procedure used by McCulloch and Pratt was to allow an animal, on each trial, to draw in either tray or both trays until he had secured food. *Thus on any one trial only one error was possible.* This eliminates, immediately, the possibility of 'repetitive' errors, and means, of course, that one of the first problems an animal has to solve with the jumping-stand procedure just *doesn't exist as a problem* in the weight-discrimination procedure used by McCulloch and Pratt. As we have seen from the discussion of our data, one of the important reasons for the superior score of the Two-Day Group was the fact that during the first two days (corresponding to McCulloch and Pratt's CSD period) the animals had made great

progress in solving the 'repetitive' problem with the result that they did not have to 'waste' errors on that particular problem when they were shifted to the final discrimination. The weight-discrimination technique, in other words, is more favorable for an immediate response to the relevant discriminanda (since the 'repetitive' problem has been eliminated by the technique used) than is the jumping-box visual discrimination technique, and therefore the pre-solution period in the former case would be significantly shorter than in the latter case. The general significance of this whole problem is of some importance.

In considering the entire question of a pre-solution period and the pre-solution hypotheses we are immediately concerned with asking the question of what are the factors which cause the adoption of any given 'wrong' hypothesis or series of 'wrong' hypotheses. In some of our previous papers we have already suggested that some of those factors lie 'within the skin' of the animal, *i.e.*, his past experience, his sensory acuity, his preferences, etc. There are, however, other factors—the external ones. The specific apparatus in which an animal is working will determine in part the kind and number of 'wrong' hypotheses the animal may adopt, and therefore, the length of the pre-solution period. Thus to use an example from the field of the history of training techniques in comparative psychology, it was held for a long time that the rat was incapable of adopting a visual-pattern hypothesis. No matter how extended the training with the Yerkes Discrimination Box the animal persisted in responding on the basis of various 'wrong' hypotheses and never could be made to respond by discriminating between visual patterns. When a slight change was made in the apparatus, it was found that the animal could and did discriminate between patterns, and his pre-solution period was shortened from 'infinity' to but a few trials.

Lashley's jumping-stand technique, while employing the improvements which enabled pattern-discrimination etc., introduced certain difficulties more or less peculiar to itself. One of these, and the most important for our consideration,

is the fact that with the jumping-box more repetitive errors occur than with the 'running-box' technique. When an animal in the Yerkes box enters the wrong alley he is allowed to retrace his steps and enter the choice point headed *away* from the wrong alley, and can then continue on into the correct alley. Very rarely does he leave the wrong alley, make a complete turn and re-enter it. On the other hand, in the jumping-stand, every time the animal makes a wrong jump, he falls into a net, is picked up by the experimenter and replaced on the jumping stand, with the same orientation he had before the jump. Possibly as a result of this, repetitive errors are extremely frequent with this apparatus, at least in the first part of the training process.

The generalization of importance here is that in attempting to describe the learning behavior of an animal, the pre-solution period, etc., we can not neglect taking the nature of the apparatus into consideration as one of the important variables.

Since, therefore, the length of the pre-solution period is longer and the number of 'irrelevant' problems which must be first solved is greater in our apparatus than in McCulloch's and Pratt's apparatus, the results of their experiment with the present one are not directly comparable.

(2) With this fact in mind it may well be that twenty-eight trials (the report of McCulloch's and Pratt's experiment is not clear as to whether this means, in terms of days, three or four days) was too long a period to use as a pre-solution period. In other words, during these twenty-eight trials the animals may have already begun to respond on the basis of weight discrimination to some extent, and would thus be somewhat comparable to our group III, rather than Group II. Had the animals been shifted somewhat earlier, different results might have been obtained.

(3) There is still another factor in their experiment which suggests that their pre-solution period was 'loaded' in favor of a possible negative transfer effect. It will be remembered that in their CSD set-up, the animals were required to pull in the *lighter* of the two weights, where in the SD set-up to

which they were then transferred they were required to pull in the *heavier* of the two. If we now assume that all other conditions being equal the animals have a preference, even though slight, for the least expenditure of energy (*i.e.*, pulling in the lighter of the two weights) we have a situation here in which the animals were shifted from a more preferred response to a less preferred one. In such a situation, assuming that the animals already had begun to react to the weight differentials, we might expect more negative transfer than would have been the case had no preference differences been involved.

SUMMARY AND CONCLUSIONS

To test the assumption that discrimination learning is a continuous and constantly cumulative process and is a direct function of the number of rewarded correct responses and punished incorrect responses, forty-seven animals, divided into three groups, were run in this experiment. The control group of animals was trained from the very beginning, on the visual discrimination used as the problem. Group II was first trained for two days on the converse set-up, and then transferred to the final problem. Group III was given the converse problem for four days before being transferred.

Contrary to the predictions based on the 'continuity' assumption, Group II and Group III mastered the test discrimination with fewer errors and less trials than did the control group.

It appears that in general the results indicate that there is no cumulative strengthening of the bonds between a given stimulus and a positive response to it with every reinforcement of such a response in visual discrimination learning. Such results make highly questionable the theoretical mechanism proposed by Spence to account for discrimination learning and also negates one of the basic assumptions found in the rational learning equations of Thurstone, Gulliksen, Gulliksen and Wolfe, and Wiley.

Instead it appears that,

(1) At first the animal goes through a 'pre-solution' period during which he responds to various 'irrelevant' stimuli and during which he solves a number of subsidiary problems.

(2) This pre-solution period, for the specific discrimination involved in this experiment, seems to occupy between two and four days of the training period. In general it is suggested that the length of such a pre-solution period is not only a function of the organism but also a function of the specific apparatus used.

(3) In the course of responding with these various 'irrelevant' hypotheses during the pre-solution period, the reinforcements and non-reinforcements of the animal's responses have little residual effect in determining the animal's behavior *as far as the relevant discriminanda are concerned*. It is suggested that only when the animal begins to 'pay attention to' or 'react to' the important stimuli does reinforcement begin to have any effect.⁵

REFERENCES

1. GULLIKSEN, H., A rational equation of the learning curve based on Thorndike's law of effect, *J. Comp. Psychol.*, 1934, 11, 395-435.
2. — AND WOLFLE, D. L., A rational theory of discrimination learning, *Psychometrika*, 1937, 2, 68-69.
3. HIGGINSON, G. D., Visual perception in the white rat, *J. Exper. Psychol.*, 1926, 9, 331.
4. KOFFKA, K., Mental Development, in 'Psychologies of 1925,' Worcester, Mass.: Clark University Press, 1925.
5. KRECHEVSKY, I., 'Hypotheses' vs. 'chance' in the pre-solution period in sensory discrimination-learning, *Univ. Calif. Publ. Psychol.*, 1932, 6, 27-44.
6. —, The genesis of 'hypotheses' in rats, *Univ. Calif. Publ. Psychol.*, 1932, 6, 45-64.
7. —, The docile nature of 'hypotheses,' *J. Comp. Psychol.*, 1933, 15, 429-443.
8. —, Hereditary nature of 'hypotheses,' *J. Comp. Psychol.*, 1933, 16, 99-116.
9. — AND HONZIK, C. L., Fixation in the rat, *Univ. Calif. Publ. Psychol.*, 1932, 6, 13-26.

⁵ This statement is not intended to mean that there is necessarily a sharp dichotomy between the pre-solution period and the rest of the learning process. It may well be that one shades into the other as far as the effect of any rewarded or punished response is concerned. That is, during the first part of the training process there may be no effect of a correct or wrong jump on the final discriminatory process, then, during a 'transition period' there may be a slight residual effect, or perhaps a maximum effect as a result of some correct responses and none from others due to such factors as 'shifts in attention,' etc., and then a greater and more constant effect.

10. LASHLEY, K. S., Brain mechanisms and intelligence, Chicago: Univ. of Chicago Press, 1929.
11. MAIER, N. R. F., Reasoning in white rats, *Comp. Psychol. Monog.*, 1929, 6, 93.
12. McCULLOCH, T. L. AND PRATT, J. C., A study of the pre-solution period in weight discrimination by white rats, *J. Comp. Psychol.*, 1934, 18, 271-290.
13. PETERSON, J., Learning when frequency and recency factors are negative, *J. Exper. Psychol.*, 1922, 5, 270-300.
14. —, Learning when frequency and recency factors are negative and right responses are painful, *Psychol. Bull.*, 1931, 28, 207-208.
15. SPENCE, K. W., The nature of discrimination learning in animals, *Psychol. Rev.*, 1936, 24, 427-499.
16. —, Discrimination habits in chimpanzee, *J. Comp. Psychol.*, 1937, 23, 77-100.
17. THORNDIKE, E. L., The fundamentals of learning, New York: Teachers' Coll. Bur. Publ., 1932.
18. THURSTONE, L. L., The learning curve equation, *Psychol. Monog.*, 1919, 26, No. 114.
19. TOLMAN, E. C. AND HONZIK, C. H., 'Insight' in rats, *Univ. Calif. Publ. Psychol.*, 1930, 4, 215-232.
20. WILEY, L. E. AND WILEY, A., Studies in learning function, *Psychometrika*, 1937, 2, 67.

[MS. received August 5, 1937]

RECENT ADVANCES IN SOME CONCEPTS OF CONDITIONING¹

BY E. A. CULLER^{2,3}

University of Illinois

1. *What is the Conditioned Response viewed as an organic function?* ⁴—It is an accepted corollary of evolutionary principles, that any response is the means whereby a living organism restabilizes processes which have been temporarily unbalanced by the stimulus evoking that response. This concept of a self-regulating mechanism has been amply documented by Cannon (1). Constancy of water content, of salt content, of blood sugar, of blood proteins, of blood fat, of blood calcium, maintenance of body temperature are but special forms of a pervasive 'homeostasis.' Admirable as these autonomic stabilizers are, they do not approach in range and flexibility the adjustive mechanisms which nature has provided in conditioning.

From training scores of dogs at Illinois, we can predict, when shock is applied to a wholly naive animal, that its

¹ Communication No. 26 from the Physiological Psychology Laboratory (Anima Hearing), Department of Psychology, University of Illinois; maintained by aid of the Research Council, American Otological Society. Special aid from the Josiah Macy, Jr. Foundation is gratefully acknowledged.

² This paper incorporates, with minor revisions, what I said about the Conditioned Reflex before a session of the American Psychological Association at the Hanover meeting, September 4, 1936. So meager a selection from so extensive a material is inevitably hard to justify; six men could each have spoken on this topic with little in common save the title. Whatever one elects to present remains a torso, without the integrity of a living organism. In behalf of the material here offered, let but one thing be said: no aspect of conditioning is included unless we have studied it in this Laboratory. I prefer not to theorize about any of these problems without first interrogating our animals. Let me particularly stress that the paper is in no sense a review or evaluation of the literature; every expert will recognize that many of the best contributions are not mentioned. Citations are introduced as needed for the argument; they are neither comprehensive nor systematic.

³ With the collaboration of W. J. Brogden, Johns Hopkins School of Medicine.

⁴ Abbreviations: US—Unconditioned (original) Stimulus; UR—Unconditioned (original) Response to US; CS—Conditioned (substitute) Stimulus; CR—Conditioned (substitute) Response.

response will include many of these features: quick gasp or yelp, hasty withdrawal of foot, adduction of tail, then whining or barking, biting or snapping at nearby objects, twisting and jerking, occasional evacuation. The whole body seems to be vigorously involved, with but little effective result. Now suppose a bell be rung just before the shock. After a few times, we witness a display of behavior (as soon as the sound begins) which seems to duplicate the actual UR; indeed, so realistic is the animal's performance that I have sometimes been misled into thinking that shock was being inadvertently applied along with bell. Never have I seen an actor give a more convincing portrayal than does the conditioned behavior of some dogs in this early stage. Similar observations have appeared elsewhere. Jones reports (9) that, in a human infant, bell plus shock were administered 3 times, directly followed by bell alone 5 times. The reactions during the last five were 'indistinguishable from those elicited by the primary stimulus'; indeed the keeper of the protocol, who observed each trial with great care, mistakenly assumed from the infant's behavior that all 8 trials included shock instead of only the first three. In this initial stage, when UR is still tentative and diffuse, the CR may indeed give a photographic reproduction. Thereafter the two diverge, UR and CR, each in accord with its own function. Let us first be clear that UR itself is not stable and definitive at the start. It alters and accommodates until some 'optimal' form has been attained, though it would be hard to define just what optimal means, or to tell when the optimum is achieved. But in any case, after a dog is often shocked, the same charge applied in the same place no longer yields this loose and wide-spread activity. It yields rather a quick, effective removal of foot, which is then slowly replaced. The UR thus means at first, readiness for something; the problem being not yet clear, the response is not yet defined; and so the animal reacts in a way suitable for a large class of stimuli. But if this improved UR were his only recourse, the animal would still be forced to wait in every case for the stimulus to arrive before beginning to meet it. The veil of the future would hang just before

his eyes. Nature began long ago to push back the veil. Foresight proved to possess high survival-value, and conditioning is the means by which foresight was achieved. Indeed this provision gave the distance-receptors most of their value. Neither sight nor sound of an approaching enemy is intrinsically hurtful; without conditionability, these exteroceptors would have lost their phylogenetic significance.

While the UR is thus developing, what is happening to CR? It also is changing; but its function is not to *react* to US itself, but to *get ready* for US, to make preparatory adjustments for an oncoming stimulus. Since this adjustive preparation will always depend on the nature of US, CR will necessarily have as many forms as the US themselves. The localized, individuated CR of a fully trained dog (foot-withdrawal just before the shock) is as unlike his early yelpings and jerkings as two acts well can be. This unlikeness of CR to UR has been emphasized by many writers; one example must suffice. Warner (20) complains of his fruitless quest for a real CR in rats. To the shock (US) they would hop frantically about and breathe rapidly; to CS (bell) they would hold the breath and wait tensely. In one setup they would escape US by scurrying underneath a fence, while CS would lead them to escape by leaping over the same fence. He also stresses the variability of CR, which changes again and again as though the rat were seeking a more adequate way to get ready for the imminent US.

The CR, in brief, is nature's way of getting ready for an important stimulus. The salivary secretion prepares the mouth for reception of food and gastric secretion for its proper ingestion. The same CS when paired with acid yields a different type of secretion than when paired with food. Conditioned foot-withdrawal enables the dog to escape an imminent shock or at least to place the limb in optimal position for reducing its effect. Conditioned lid-closure protects the cornea from a blow or jet of air. Conditioned iridic reflex saves the retina from undue stimulation. Conditioned galvanic skin-response, according to Darrow (2), prepares for seizing and manipulating the stimulus, and so on.

So omnipresent is this mechanism, at least in higher forms, that *every* S has potentially *two* functions: first, to adjust the organism to itself, and secondly, to get the organism ready for the stimulus which customarily is next in line.

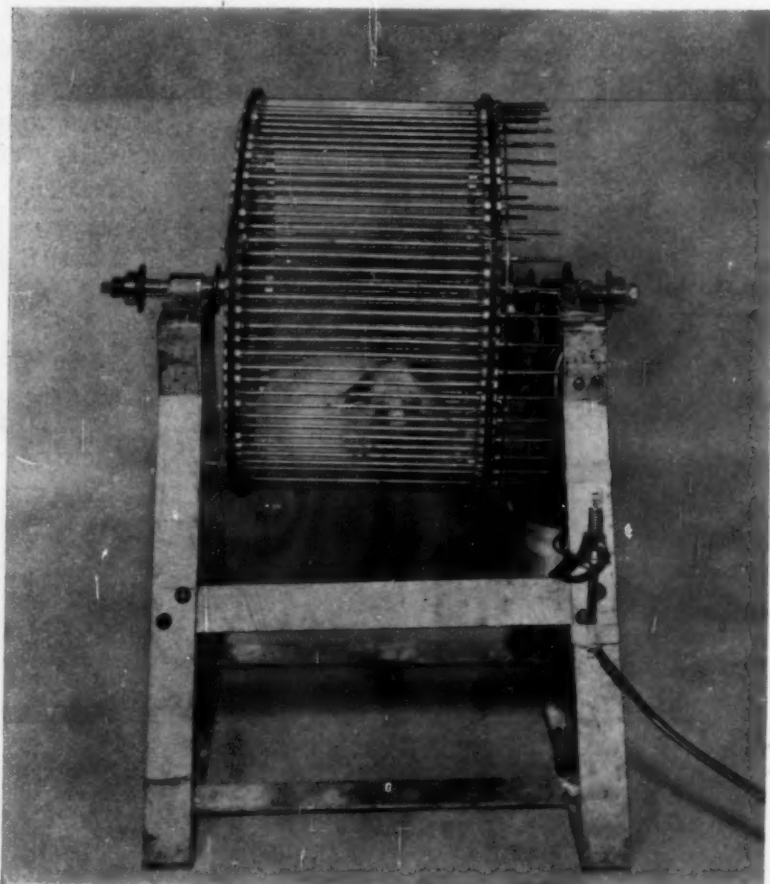


FIG. 1

The preparative character of CR is illustrated by the material of Dr. Brogden who used a modified activity-cage. It is so devised that the animal, upon turning the cage an inch or more when the sound begins, escapes the shock by breaking the high-voltage circuit through a pendulum-switch. Four guinea pigs in group *A*, were trained by the methods

commonly employed in this laboratory, and progressed, as the chart (Fig. 2) shows, rapidly to a perfect performance (dotted line). Twenty-five trials are given each day, the ordinates being therefore percentages of that number. The four pigs required respectively 5, 7, 8, 13 days or 125, 175, 200 and 325 trials to reach a perfect score. The figures were combined into a single curve by the so-called Vincent-method. The four pigs of group *B* (continuous line) however, though exposed to exactly the same situation (same sound, same shock, same test-environment) were not allowed

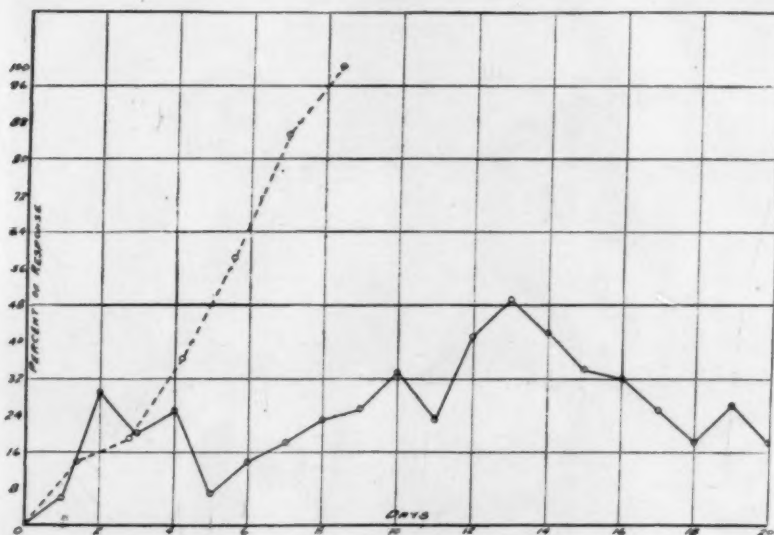


FIG. 2

to escape the shock by reacting; on the contrary, they were shocked every time, whether they turned the cage or not. We see that they began precisely as did the other four and gave every preliminary indication of advancing in the same way to full conditioning, but soon the incipient adaptation broke down. The pigs became erratic, now rising, now falling in random fashion from day to day. After a promising start their behavior has become confused, the prey of competing impulses; by end of five hundred trials, the picture still remains the same. What is wrong? Obviously the CR

cannot here perform its normal function of preparing for the imminent shock. No preparative response is possible; nothing is gained by reacting, since the shock comes anyway. So they literally 'sit tight' when the bell sounds; they hold the breath and wait tensely for the shock. It would be hard to fancy a sharper contrast. When CR is allowed to perform its normal preparative function, it develops neatly and quickly to full maturity, even in animals so notoriously stupid as guinea pigs. When this normal function is denied by balking the pigs' efforts at accommodation, their behavior bogs down into a morass of competing impulses.⁵

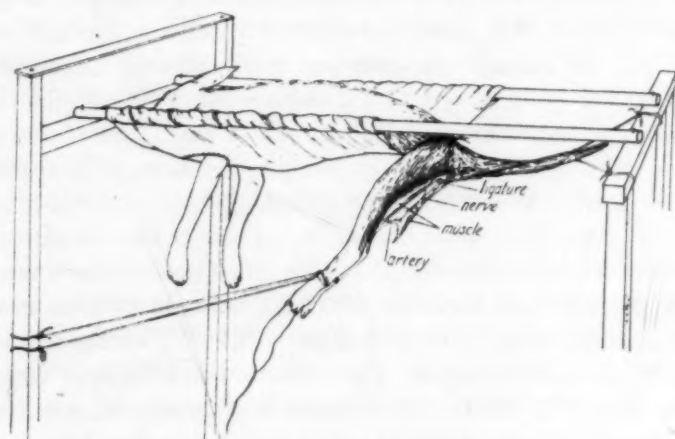


FIG. 3

We may well discuss this matter at length since it is basic to any true appreciation of the CR and its organic function. Consider this experiment. In a dog under general anesthesia, we incise the skin from lower hip (ischium) down to knee. Clearing away connecting fascia, we hook the finger under a cylindrical muscle (*M. semi-tendinosus*), formed like a frankfurter, which originates in tuberosity of the ischium (the bony prominence on which one sits when seated) and inserts into the medial side of tibia just below knee.

⁵ Incidentally, we think this graph justifies our method of punishing the animal only when he *fails* to react and rewarding him by permitting him to escape punishment when he does react.

By cutting the large, wide tendon, we have an isolated muscle with intact nerve- and blood-supply for experimental use. Anesthesia being discontinued, we inject enough morphine sulphate to provide light sedation without appreciably depressing neural function. The animal's body is so supported as to leave the operated limb and muscle free in air for manipulation and observation. A clamp attached to the cut tendon provides a light load for the muscle to lift. Two stimulating electrodes are now clipped to the toepads. With a potential of 30-45 volts, the exposed semitendinosus contracts sharply. Note that the shock is impressed far from the muscle, so that it is activated wholly through its usual nerve-supply. We now proceed to condition in the usual way: bell or buzzer for two seconds, directly followed by shock to toe. From 5 to 50 combinations of bell and shock are needed until the *first* CR appears; but after a hundred trials (more or less), the proportion of CR will approach 100 per cent. We shall refer to this preparation again, but are now concerned with the early stages, before conditioning has been stably achieved. If the muscle is fully extended (relaxed), the bell initiates a mild tensing which is relieved by the sharp contraction due to shock. This is the usual picture, the CS initiating the contraction which is consummated by US. When the muscle is thus under contraction by the previous shock, as much as ten or twenty seconds may be required until it fully relaxes. Suppose we again sound the bell while the muscle is still taut from the previous shock. Does it then contract some more in anticipation of the shock? No. Does it hold still? No. On the contrary, it relaxes and thus meets the US in a slightly tonic condition. This phenomenon deserves more analytic attention than I have yet been able to give it, but in any case it is authentic. This CR has thus *two* forms which are 180° out of phase, contraction and relaxation. The muscle seems to assume a medial degree of contraction, in preparation for US. Neither extreme tension nor yet complete flaccidity is optimal for a muscle which is to be in readiness; like the runner on his mark, a moderate and labile tonus is the best preparation for the gun.

Whether or not my interpretation be acceptable, we at least have here clear evidence that CR is something more flexible than a mere mechanical replica of UR. While UR is indeed dominant and CR is subservient to it, each has its own role and each develops in accord therewith.

Let us carry the analysis forward to certain applications or issues of conditioning. In recent years we have learned that the most useful CRs have to do not with *external* but rather with *internal* CSs. Consider the well known experiment of Hudgins (7). Sound of bell, which normally dilates the pupil, was so conditioned, by combination with strong light, as to cause marked contraction. The conditioned stimuli included however not bell alone, but certain words spoken aloud by the subject. Indeed, whispering the word 'contract,' or even subvocal repetition ('thinking' of this word) was eventually able to elicit the reflex. This demonstrates that intraorganic changes may serve as CS; and suggests that *any* interoceptive mechanism whatever may be employed for establishing a conditioned pathway to *any* response. As Hunter and Hudgins remarked, we have here the foundations of voluntary behavior; autonomic processes, which are natively just as independent of lingual mechanisms as is salivation independent of a ringing bell, become so conjoined with them as to be elicited whenever the words are pronounced. Volition thus becomes a mechanism for using certain available lingual or manual acts as the conditioned cues for other behavior.⁶ The soundness of this general position is confirmed by recent evidence from Razran (14). With human subjects salivation was secured not merely by using the common US, such as eating pretzels or mint wafers, but also by observing someone else eat pretzels, and finally by thinking of eating pretzels. His results confirm and supplement Hudgins in a significant way: most of his USs were actually CSs which had been earlier established in his subjects. What Hudgins found to be possible is here

⁶ I here assume Hudgins' findings to be correct, although Steckle and Renshaw (16) and later Steckle (17) again, failed to confirm them; compare Hudgins' defense (8). Whatever proves to be the truth in this particular case, I still adhere to the theoretical position above stated.

utilized in practise: these verbal stimuli, formerly conditioned, are now employed as the means of conditioning other stimuli. Nature by providing the organism with interoceptors has vastly extended its resources of accommodation to the future. We need not wait until we see the angry bear before beginning to escape; we can plan by interoceptive CRs to protect ourselves long before any bear is in the field of vision. In other words, we can imagine and foresee.

To summarize this section. May CR be regarded as a true replica of UR (the same R with a substitute cue) or is it instead a brand new activity? This question has elicited much fruitless debate because it was illy phrased. Both views can be defended because they view the CR at different levels of development. Our answer is that CR (1) begins as a copy of UR and then (2) grows into something different. In the first stages, it may be indistinguishable from UR; indeed with decorticate preparations it remains indistinguishable throughout. Normally however CR differentiates into a specific preparation for the oncoming US. This essentially preparative character of CR appears on every hand. It relaxes or contracts the muscle, according to which activity is needed to bring it into a state of medial tonus for reacting to US. When this normal function is thwarted, we find the CR degenerating into confused and disarticulate behavior. CR thus becomes the means of foresight. By utilizing distance-receptors, we need not wait for actual incidence of a noxious US before initiating defensive measures. But this applies even more to intra-organic CS; long before any external CS can activate the distance-receptors, we can by self-initiated internal CS make preliminary adjustments to a US which is still far in the future.

2. *The concept of latency.*—The concept of CR as the organism's way of getting ready for US sheds some needed light on latency. By this is meant, of course, the time which elapses from application of the CS until the effector starts to reply. But if CR is indeed a 'primer' for US, it should appear in the best temporal relation, whatever that may be, to the dominant stimulus. The tendency of CR to appear

just prior to US was noted by Schlosberg (15), Wendt (21), Hilgard (6), and others. Workers a few years back seemed to be surprised that CR did not automatically arise *at once* after CS, but they failed to recognize that it is US which determines when CR is to appear; the whole pattern is oriented in relation thereto. Consider Fig. 4 from Hilgard.

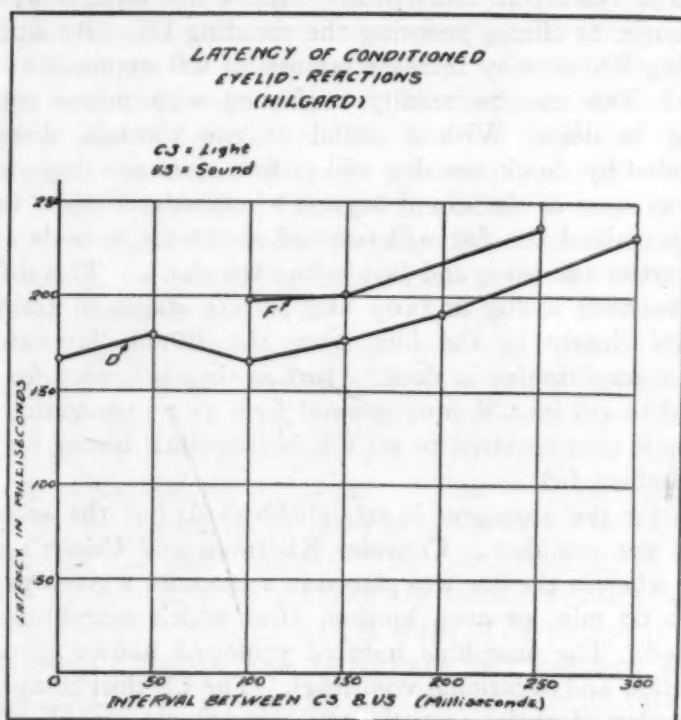


FIG. 4

He used a sharp sound to evoke the wink-reflex and then conditioned this eyelid-closure to a flash of light. When light and sound are coincident (zero-interval), 167 ms elapse before CR begins. With sound delayed more and more up to 300 ms, latency also rises up to 231 ms in Subject D. Likewise in Subject F, latency rises from 198 ms when CS precedes US by 100 ms to 236 ms when it precedes by 250 ms. The trend is clear: CR tends to keep just ahead of US; latency increases as US is delayed. It is evident that the

only latency which has real meaning is liminal latency—the *minimal* time in which CR can be aroused. As US crowds closer to CS, the CR ‘hurries up’ until it reaches a functional limit, which here is near 165 ms. When the interval between CS and US falls below this value, the CR’s normal preparatory function becomes impossible; hence we find that the latencies from 0 to 150 are all much alike. Above this time, however, they begin to climb, pursuing the receding US. By simply delaying US we may increase latency of CR as much as we please. This can be readily confirmed with motor conditioning in dogs. With a sound of two seconds, directly succeeded by shock, the dog will at first start reacting vigorously as soon as the sound begins; whereas later when thoroughly trained the dog will respond about 1.5 seconds after the start of the tone, and just before the shock. This difference between a dog in early and in late stages of training appears clearly in the film from the Illinois laboratory, ‘Motor conditioning in dogs.’ Just as time is needed for the animal to get its CR into optimal *form* as a preparation for US, so is time required to get CR into optimal *timing* for the approaching US.

So far the argument is straightforward; but the account is not yet complete. Consider Kleitman and Crisler’s (10) study wherein the dog was placed in a stock for a given period (15 to 60 min. or even longer), after which morphine was injected. The morphine induced profound nausea (profuse salivation and occasional vomiting). The CS thus comprised a complex of optic, acoustic and somesthetic stimuli which bombarded the dog as he stood in the stock. In the early stages, the animal would “begin to secrete in the latter part of the test period,” but as CR became better established, secretion began to pour out earlier. Eventually when CR was fully established, the “flow reached a maximum value as soon as the animals were placed in the stock.” The CR was not ‘delayed’ even when the US (morphine) was delayed for two hours after the animal was stocked. This phenomenon seems to be precisely opposed to what we encountered in motor conditioning. The whole problem (in one case CR

delays and thus approaches US, in another case CR speeds up and thus approaches CS) needs more attention than it has yet received. The key is again, in my opinion, the concept of preparatory priming. When the animal can solve the situation, the CR is delayed until it is time to get ready for US; otherwise it falls into the confused repetitive behavior which characterizes emotional (sub-cortical) activity. The morphine-dogs can neither escape nor ameliorate the drug's effects; despite all their preparatory salivation the nausea recurs with daily regularity. At this inchoate level, CR is a close replica of UR; hence CR_1 becomes itself the stimulus for which the animal prepares by moving a step ahead to CR_2 . Each stage thus becomes the US for a stage preparatory to it. In a vain quest for escape from its maladjustment, the animal keeps moving ahead until it starts salivating as soon as placed in the stock.

To sum up. The only latency which has a single, defined meaning is *minimal* latency, the ultimate speed with which the conditioned arc can function. To determine the true latent period of CR it is necessary to use reflexes which are as nearly as possible simultaneous. "The true latent period of a CR is really of the same order as is the true latent period of reflexes in the lower centers of the brain and spinal cord." (13, p. 105.) All other latencies are contingent upon variable factors: interval from CS to US, stage of development of CR and others. Unless these factors are fully specified, latency is meaningless or misleading. In accord with its functional role, CR tends normally to appear just before US, that is to say, long enough to get ready for it, but when no adequate adjustment is possible, a kind of neurosis ensues. A dog which has not yet solved the bell-shock situation may become overactive; he then paws the grid excitedly, up and down, without regard for sound or shock. The animal resorts to the crude repetitive sequences which characterize sub-cortical behavior: US is the basis for CR_1 , which in turn is the basis for CR_2 and so on. Since the whole phenomenon depends on individual adjustment, great variations appear.

3. *Extinction and higher order conditioning.*—One of the most persistent difficulties in our attempt to apply the principles of conditioning more widely in the field of learning is concerned with extinction and higher-order conditioning. A Pavlovian animal cannot get away by more than one or two steps from the US which initiated the series; whereas the growing child fashions day by day, year by year, a complex concatenation of acquired knowledge and skills, adding one unit to another in endless sequence. Thorndike, referring to one of Pavlov's cases of experimental extinction, says: "Ordinary learned connections do not act in this way. If a child has learned to respond to seven times nine by saying 'sixty-three,' and we ask him every two minutes, 'How much is seven times nine?' he does not as a rule become more and more halting and inaccurate and, after a dozen repetitions, fail" (18, p. 109).

I wonder whether Professor Thorndike ever tried what he here suggests: to ask a child or an adult, over and over with relentless regularity every two minutes, how much is 7×9 ? If my experience is typical, something like this may be expected to happen. For one or two times, the respondent will indeed reply, 63. Then as the question continues, divergence will appear. One with a grimace of surprise or reproach will hold his peace; another may ask, "Is this a new psychological test?" A child, who knows you just well enough to be impudent, may make a face at you. Very few indeed, unless overawed by your eminence as a professor of psychology, will continue faithfully to reply, 63. If you ask, why should anyone continue repeating 63 twenty or a hundred times over, let me ask in turn, why do we expect a dog to go through the pantomime of carefully withdrawing his paw time after time from a harmless grid? We seem to be surprised that our animals are less stupid than we think they ought to be. It is clear that the dog, when he stops retracting his paw from an uncharged grid, has no more 'forgotten' how to do it than has a child forgot '63' when he fails to answer our question a dozen times over.

The whole question, we think, has been clarified by an

experiment with several dogs (5). After a CR to a 1000-cycle tone was thoroughly established in the usual way by shock-to-paw, a second stimulus (light) was applied together with the tone; but now the animal was shocked, not on forepaw as heretofore, but on left side of thorax. By a simple switching arrangement, the animal could now escape the thoracic shock by simply withdrawing its paw from the grid just as it earlier avoided shock-to-paw by the same response. Before long the animal responded to the light alone, without the conditioned tone; the light was then combined with a squirt of water on the nose (failure to react being again punished by shock on thorax) until he always responded to water alone. Then an electric bell was presented along with water until the bell was conditioned; and finally an electric fan was conditioned by combining it with bell. We then stopped, it being obvious that the transition from one CS to the next was getting easier and quicker at every step. The primary shock-to-paw was not applied a single time with any S except the first (tone) during the whole period of higher order conditioning (42 days); in all other cases, only thoracic shock was used.

We have here in my opinion the explanation of the alleged discrepancy between learning and conditioning. All we need in order to proceed indefinitely with higher order conditioning is adequate incentive. In school and society, the child is activated by incentives which are fresh each day, incentives which, like the thoracic shock, motivate the acquisition of new and higher orders. A telephone message is carried across the continent by providing booster-stations along the way to restore the power lost in transit. If it were restricted to the energy which is applied at New York, the message never would reach San Francisco. Likewise, in higher order conditioning, we must provide boosters along the way; we cannot expect that some primitive shock which occurred in infancy will provide enough incentive to motivate conditioning all the rest of the subject's life.

Unless some incentive for response be at hand, the act will inevitably be delayed until an incentive does appear.

If *A* is the signal for shock, *A* will indeed elicit the CR; if *A* merely heralds the coming of *B* which in turn signalizes the shock, we may get the CR moved ahead to *A*; but it would violate the whole preparative conception of CR were an animal to start reacting with *A* when the shock did not occur until *Z*. There is no more reason why the dog should begin reacting with *A*, when the shock which makes the act appropriate does not appear until *Z*, than for me to proceed now (Thursday) to the railway-station because I intend to take a train next Saturday.

The US (shock) plays a dual role in conditioning (*cf.* 4). (a) It determines the character or pattern of the response (leg-flexion). (b) It provides the incentive or drive needed to actuate this response-pattern. We have just seen that motivation can be transferred to a new stimulus (thoracic) while the pattern is still being determined by the original stimulus (pedal). By thus separating those factors which provide the response-pattern from those which provide the drive, we get the set-up found in ordinary learning.

1. Conditioning

- a. Pattern of response—due to US
- b. Incentive to respond—due to US

2. Learning

- a. Pattern of response—due to US
- b. Incentive to respond—independent of US; commonly provided by social rewards and penalties

When shock is removed, the dog has no further incentive to continue withdrawing his paw; but in learning, the case is very unlike. The incentives to remember 7×9 do not proceed from the printed page which provides the pattern of response. They are supplied by teacher and parent, by classmates, by social rewards for industry and penalties for indolence. Lashley (11, p. 486), Leeper (12), and Tolman (19) have emphasized the distinction between *forming* a pattern and *utilizing* a pattern; it is the latter that concerns us here. In extinction the pattern is intact but is not being

utilized. Schlosberg (15) observed that the CR often displays the properties of a split-off element of behavior, a kind of dissociated unit. Dashiell (3) likewise states that conditioning in its pure form is the function of a particular segment of the organism. Being thus remote from the main sources of energy in the organism, these 'pure' CRs are peculiarly susceptible to rapid extinction. No wonder they lack stability and depth. It is rather ironical that in some circles one still cannot qualify as a true 'conditioned reflexer' unless one confines his attention to those isolated units which lie far beyond the range of voluntary control; which means, far beyond the integrated neural channels by which available energy can be canalised where needed. When pupillary contraction or secretion are made voluntary, as Hudgins reported, we have done more than merely establish a new pattern; we have opened a channel to iridic muscle or parotid gland from the vast store of available neural energy in the cerebrum. From this general store, any learned act can draw the sustenance needed to insure its indefinite utilization. As soon as any CR is made 'voluntary,' it gains access at once to the same general store of central energy; and thus operates by precisely the same laws as do the phenomena of ordinary learning.

In this connection we must consider the concept of inhibition. The term implies that a given process is being held down, like Jack-in-a-box, by some force which sits on the lid. If the force be withdrawn, the lid flies open and Jack bobs up again. Consider how fears were overcome in the well known experiments by Watson and Mary Cover Jones. Peter is afraid of a rabbit. While he eats his bread and milk for lunch, a rabbit is placed within view but safely barred at the other end of the room. Next day the rabbit (in the cage) is moved nearer and next day again nearer, but it is always kept beyond the limen of tolerance; finally, as a result of this conditioning, Peter holds the rabbit in one arm while he wields his spoon with the other. Shall we say that Peter's fear is now 'inhibited,' with the implication that the fear is still a living thing, its force latent as in a coiled spring,

waiting only until it may leap forth again and startle its hapless victim? On the contrary, the fear no longer exists after re-conditioning has occurred. The patterns which once underlay its baleful power are replaced by others. This concept of inhibition, it seems to me, has complicated and confused our thinking. First we have conditioning, then extinctive inhibition, then dis-inhibition, when the inhibition is itself inhibited. When the inhibited inhibition is next inhibited, do we have dis-dis-inhibition? Why not stay with the simple and positive concept of conditioning from the start? Every form of inhibition is re-conditioning to a new situation with *new* patterns and *new* incentives. The one concept covers all; and thereby simplifies terminology and clarifies thinking. A dog is conditioned to the bell; the shock is then discontinued. Is the CR 'inhibited'? Why not say he is now conditioned to a new situation? Without shock, the bell becomes a *safety*-signal even as with shock it was a *danger*-signal. Consider the material from Dr. Brogden's experiment. Four dogs are first conditioned to a bell and the CR is then extinguished, whereupon the test begins. Two dogs are first given 400 additional trials without shock, so as to make the extinction even more complete, while the other two are reconditioned to the bell directly after extinction. All four dogs are then reconditioned and again the CR is extinguished; whereupon the two series are reversed (abba). The question is whether more incentive (shock) is needed to overcome the prolonged extinction than the bare extinction? The table gives the answer:

TABLE I

No. of dog	1	2	3	4
No. of shocks for reconditioning after prolonged extinction (400 trials).....	15	11	19	10
No. of shocks for reconditioning after bare extinction to zero....	4	4	8	5

Is it not clear that we are here, in these 400 extra trials, conditioning the animal more strongly to keep his foot on the grid? The bell without shock becomes ever more a safety-

signal (stay *on* the grid!), even as, when combined with shock, it becomes an ever stronger danger-signal (get *off* the grid!). Keeping the foot on the grid is not mere failure to act but may be just as positive a response as withdrawing it. A dog under extinction will, more and more, *press the foot down* against grid while the tone is sounding. This is just as true conditioning as is retraction of the paw. Why, then, confuse the issue by giving it a different name?

The rate of extinction, as of conditioning, is largely a matter of incentive. Finch and Culler (4) trained some dogs with tone and shock. At every trial a buzzer, placed in parallel with the grid, sounded sharply for the exact duration of the electric charge. The animals were repeatedly reconditioned and extinguished; extinction being either *with* or *without* the buzzer (no shock in either case). When the bell was always followed by the buzzer, extinction required 14.9 periods; when bell was followed by silence, 4.0 periods were required. In the latter case, extinction (inverse conditioning) occurred rapidly because keeping the feet on the grid is easier than retracting them. There is no contrary incentive to counteract this labor-saving propensity. But when the buzzer sounds, it serves as a substitute-shock; hence we have two incentives which are 180° out of phase, one tending to quiescence, the other to flexion; the flexion-incentive is buttressed and inverse conditioning is slow. What will happen if the 'extinctive conditioning' is more strongly motivated—not alone by inertia but by added incentive as well? Dr. Brogden conditioned a dog by shocking it on the paw when it failed to flex the leg (direct conditioning up to 100 per cent level); he then began extinction by shocking it on the thorax whenever it *did* withdraw the paw. Direct conditioning now has the usual incentive, while inverse conditioning has a much stronger incentive (thoracic shock).

Here we see that negative conditioning, when duly motivated, proceeds much faster than positive; and this in turn faster than non-motivated extinction. Extinction fits readily into the larger concept of conditioning; the two differ only in *phase*. Being opposite in phase to conditioning,

extinction may conveniently be styled 'inverse' or 'negative' conditioning.

TABLE II

1. Dog is first conditioned to 100%, leg-flexion.	
2. Extinction with thoracic shock.....	75 trials
3. Re-conditioning.....	250 "
4. Extinction without thoracic shock.....	325 "
5. Re-conditioning.....	200 "
6. Extinction without thoracic shock.....	350 "
7. Re-conditioning.....	150 "
8. Extinction with thoracic shock.....	75 "

Means

Extinction <i>without</i> thoracic shock.....	337.5 "
Extinction <i>with</i> thoracic shock.....	75.0 "
Re-conditioning.....	200.0 "

SUMMARY

The argument may be summarized in three propositions.

1. The CR begins its career as a copy of UR; not exact but labile, often differing from it in amplitude and timing. They are members of a common species, equivalent responses to different stimuli. Thereupon the two diverge, each evolving by a process of accommodation to the other; until, when fully matured, CR prepares and starts the act which is consummated by UR.

2. The latency of CR is functional rather than anatomical; that is to say, it depends very little upon the distance of neural pathway to be traversed and very much upon the position of UR. Because of its preparative function, CR comes to rest where it can 'prime' the act which is completed by UR. Latency has therefore but little meaning apart from the primary stimulus.

3. Whereas forgetting implies rupture or decay of the learned pattern, extinction occurs when the pattern, though still intact, is not being adequately activated. The previous pattern (get *off* the grid!) is no longer being activated because another pattern has arisen during extinction (stay *on* the grid!). These two patterns being functionally 180° out of phase, extinction may properly be called *negative* or *inverse*

conditioning. By this treatment extinction, and inhibition generally, find their theoretical locus in the concept of conditioning; their relations are thus clarified and simplified.

REFERENCES

1. CANNON, W. B., *Wisdom of the body*, New York: W. W. Norton, 1932.
2. DARROW, C. H., The galvanic skin reflex (sweating) and blood pressure as preparatory and facilitative functions, *Psychol. Bull.*, 1936, **33**, 73-94.
3. DASHIELL, J. F., Survey and synthesis of learning theories, *Psychol. Bull.*, 1935, **32**, 261-275.
4. FINCH, G. AND CULLER, E., Relation of forgetting to experimental extinction, *Amer. J. Psychol.*, 1935, **47**, 656-662.
5. ———, Higher-order conditioning with constant motivation, *Amer. J. Psychol.*, 1934, **46**, 596-602.
6. HILGARD, E. R., Conditioned eyelid reactions to a light stimulus based on the reflex wink to sound, *Psychol. Monog.*, 1931, **41**, no. 184.
7. HUDGINS, C. V., Conditioning and voluntary control of the pupillary light reflex, *J. Gen. Psychol.*, 1933, **8**, 3-51.
8. ———, Steckle and Renshaw on the conditioned iridic reflex: a discussion, *J. Gen. Psychol.*, 1935, **12**, 208-214.
9. JONES, H. E., Conditioning of overt emotional responses, *J. Educ. Psychol.*, 1931, **22**, 127-30.
10. KLEITMAN, N. AND CRISLER, G., Study of a salivary conditioned reflex, *Amer. J. Physiol.*, 1927, **79**, 571-614.
11. LASHLEY, K. S., Learning: III. Nervous mechanisms in learning in 'A Handbook of General Experimental Psychology,' Worcester: Clark Univ. Press, Chap. 10, 456-496.
12. LEEPER, R., The role of motivation in learning, *J. Genet. Psychol.*, 1935, **46**, 3-40.
13. PAVLOV, I. P., *Conditioned reflexes*, (Trans. by G. V. Anrep), Oxford: Oxford Univ. Press, 1928.
14. RAZRAN, G. H. S., Conditioned responses, *Arch. Psychol.*, 1935, **28**, no. 191.
15. SCHLOSBERG, H., Investigation of certain factors related to ease of conditioning, *J. Gen. Psychol.*, 1932, **7**, 328-42.
16. STECKLE, L. C. AND RENSHAW, S., Investigation of the conditioned iridic reflex, *J. Gen. Psychol.*, 1934, **11**, 2-23.
17. STECKLE, L. C., Two additional attempts to condition the pupillary reflex, *J. Gen. Psychol.*, 1936, **15**, 369-377.
18. THORNDIKE, E. L., *Human learning*, New York: Century, 1931, 109-110.
19. TOLMAN, E. C., The law of effect, *J. Exper. Psychol.*, 1933, **16**, 463-470.
20. WARNER, L. H., Experimental search for the "conditioned response", *J. Genet. Psychol.*, 1932, **41**, 91-115.
21. WENDT, G. R., Analytical study of the conditioned reflex, *Arch. Psychol.*, 1930, **19**, no. 123.

[MS. received August 12, 1937]

A CRITIQUE OF THE 'GALVANIC' TECHNIQUE

BY CHRISTIAN A. RUCKMICK

University of Iowa

In the course of continuous investigation of an entire series of problems it is advisable to take stock of the data not only at short intervals, as in the development of a single publication, but also at relatively longer periods. It therefore occurred to me that in connection with about a dozen directed dissertations and researches of my own, it might be serviceable to note some specific generalizations and derived principles which emerge from these studies. Besides we can glean additional verification of some of these statements from other researches done outside of our laboratory in so far as these are coordinated with ours in the type of problem attacked. Unfortunately the title of the paper must still bear the term 'galvanic' when the only excuse for it lies in the fact that we continue to use some form of galvanometer in our work. I much prefer the term 'electrodermal' as more expressive of the fundamental factors involved in the technique.¹

First of all we are faced with some basic facts that come out clearly from our investigations. Obviously, like most if not all other bodily responses, the electrodermal response is not exclusively affective in indication. Since it is probable from Darrow's recent deductions² that the parasympathetic, as well as the sympathetic, branches of the autonomic nervous system are connected with the sweat glands, we can look for an even more widespread effect from many sorts of mental and bodily activity. Besides, from the psychological angle, the various types of purely mental activity may be more or less affectively colored—even if from no other source than that of

¹ C. A. Ruckmick, Terminology in re 'psychogalvanic reflex,' *Psychol. Rev.*, 1933, 40, 97-98.

² C. W. Darrow, The palmar galvanic skin reflex (sweating) and parasympathetic activity, *Amer. J. Psychol.*, 1936, 48, 522-524.

motivation and drive. We do not need Wundt's systematic position or Carr's theory of judgmental attitudes from object to subject to assume that many kinds of mental processes may have an affective component or aspect. So far as this is possible, we have placed some of these confusing factors under relative control. Thus Schaefer,³ Jeffress,⁴ White,⁵ and others have studied the kinaesthetic or motor factor and Richter⁶ and Ruckmick⁷ have studied allied, though psychophysiological factors such as sleep, relaxation, and fatigue. In general it appears that electrical resistance is increased during unfatigued movement, relaxation, and sleep, and decreased during fairly complete fatigue as well as in affective responses.

With many of these data at hand, it is possible, however, to make a somewhat definite assertion. Whenever the experiments are carefully done and verbal reports are compared with the EDR, we can say that the length of the period of latency, the rapid decrease of electrical resistance, and the relatively short period of change in that resistance, are indicative of an affective response. The analogy still continues to hold, as illustrated in my recent book, that an affective response, especially an emotional response, shows a 'cliff' pattern in the photographed deflection, whereas a motor response shows a 'mound' pattern.⁸ These results are also indirectly verified through as yet unpublished studies made by means of the EDR.⁹ The emotional excitement attached

³ B. Schaefer, The effect of movement on the electrodermal response, *Psychol. Monog.*, 1936, 48, No. 2, pp. 57-73.

⁴ L. A. Jeffress, Galvanic phenomena of the skin, *J. Exper. Psychol.*, 1928, 11, 130-144.

⁵ M. M. White, Relation of bodily tension to electrical resistance, *J. Exper. Psychol.*, 1930, 13, 267-277.

⁶ C. P. Richter, Physiological factors involved in the electrical resistance of the skin, *Amer. J. Physiol.*, 1929, 88, 596-615.

⁷ C. A. Ruckmick, Emotions in terms of the galvanometric technique, *Brit. J. Psychol.*, 1930, 21, 149-159; Fatigue in terms of the electrodermal response, *Brit. J. Psychol.* (in press).

⁸ C. A. Ruckmick, The psychology of feeling and emotion, New York: McGraw Hill, 1936, pp. 366-367.

⁹ Reported in 1936 at the Christmas meeting of the American Association for the Advancement of Science.

to the telling of lies has the characteristic pause and rapid swing of the optic lever common to affective responses.

As an incidental by-product of several researches comes the inescapable inference that qualitative distinctions between pleasantness and unpleasantness can not be made in terms of the EDR. A recent study by Jurgensen, in which he compared the EDR with the appraisal of the affective coloring of words on a seven-point scale by the procedure of impression, again definitely corroborated this conclusion. On the other hand it appears to be increasingly possible to establish some sort of gradient, on the basis of the EDR for any one subject, from the more primitively developed emotional responses in a phylogenetic schema to the cognitively more highly developed emotional responses. To be more concrete: a most vehement jealousy can hardly ever equal in amount of deflection the most violent anger or fear. The earlier indications of this fact by Patterson in her study of surprise¹⁰ and by Darrow's study of sensory *vs.* ideational material¹¹ tend toward this direction. Aside from the quantitative aspect of the intensity of the feelings, apparently the EDR is more definitely correlated with the genetically older and biologically more primitive feelings and emotions. When the feelings involve cognitive components that lean more and more toward ideational aspects, they correspond less and less with the amount of the EDR.

One important point is probably beyond dispute. When observers are adequately trained to report their affective experiences, it becomes more firmly established by each succeeding investigation that the intensity of the reported feeling corresponds to the decrease in electrical resistance as manifested by the EDR. Patterson, for example, found eight series of correlations between the two procedures, ranging from .527 to .879 by the rank-order technique and others have published similar figures on a relatively small number of

¹⁰ E. Patterson, A qualitative and quantitative study of the emotion of surprise, *Psychol. Monog.*, 1930, 40, No. 1, p. 105; see also C. A. Ruckmick, *Psychology of feeling and emotion*, 1936, p. 369.

¹¹ C. W. Darrow, Differences in the physiological reactions to sensory and ideational stimuli, *Psychol. Bull.*, 1929, 26, 185-201.

cases.¹² We have just completed a study in which the deflections, as photographed by the Greenwald type of galvanometer, correlated fairly consistently with the degree of feeling reported on a seven-point scale. Since the amount of excitement, and not the quality of the excitement, *e.g.*, pleasant *vs.* unpleasant, is significant in this connection, we do not have as many steps in the gradation as the seven-point scale might otherwise indicate. Nevertheless, by using the biserial coefficient of correlation between the two sets of data, *i.e.*, the results of subjective rating and the amount of deflection, on two thousand items with twenty Os, Jurgensen found that there was a mean *r* of .44 ranging from .17 to .69. Also in 90 per cent of the individual cases the mean deflection of each observer was indicative of the assigned amount of hedonic tone in his responses, but in this second instance the relatively small number of cases does not warrant a very great degree of certainty on that particular point.¹³

A considerable number of other investigations have either assumed this statement to be true or, where reliable reports of observers were available, they have furnished evidence in support of the general principle. From the strictly behavioristic point of view, it is often generally supposed that the amount of bodily disturbance is an index to the amount of affective excitement. Logically in those instances, of course, the last statement is redundant, since the two terms on the opposite sides of the equation are identical, *i.e.*, emotional excitement equals bodily excitement. But it is well to point out that the mental factor is also quantitatively in agreement. In lie-detection the same assumption is often tacitly made, since the amount of the indicated disturbance, whether it be systolic blood pressure, respiration, the EDR, or some other bodily aspect is taken as the cue. So analytical reports under controlled conditions are essential because they furnish proof of the assumption. In this connection, however, we must not overlook the fact that in very accurate work of

¹² *Op. cit.*, pp. 95-96.

¹³ C. E. Jurgensen, A comparison between electrodermal response and hedonic tone, Master's thesis, Univ. of Iowa Library, 1937.

the most critical type, the tendency is more in the direction of making diagnoses, not in terms of relative amounts of disturbance, but in terms of the pattern of the response curve, especially in its recovery or return phase. As I have pointed out in an earlier paper, the matter becomes therefore unfortunately not simply statistical in the diagnosis, but perceptual in nature. Indeed, it becomes more obvious, as research in this area advances, that we may have to look for more comprehensively integrated patterns of many, if not most, physiological activities of an individual body, that are simultaneously recorded, in order to obtain a completely diagnostic picture. Not only are these physiological systems mutually interactive, but there may be large individual differences in the patterns, a fact that is already being sufficiently indicated in our research. Since in the EDR technique we are dealing primarily with an autonomic neural mechanism, the next step should be in the direction of coordinating this technique with electrocardiograms and with electroencephalograms.

These are some of the fundamental principles which should become increasingly useful to all workers in this area. They mark the high spots in research work up to the present time. Many other detailed data, along the line of individual, age, and sex differences, of clinical disturbances, of conditioning, of methodology, technique, and apparatus, and of constancy and reliability of results, could be discussed. But it is highly important that we begin at the bottom with the more fundamental considerations and move on again toward the more specific details, from which these generalizations were derived in the first place, with greater confidence, and in some cases with greater care, than before. I do not claim that all the principles outlined above will stand without reservation for all time, but that at least for the present they seem to be tenable.

We come now to some negative points. We all know the value of telling at least some students what *not* to do in the laboratory, or how to fail in the laboratory course, or what psychology is distinctly *not*. In spite of the fact that the

psychology of advertising and of suggestion in general has long tabooed negative statements, with proper reservations and a reasonable degree of intelligence, we may proceed with caution and scan some assumptions which have proved to be erroneous and which may lead to systematic confusions. In a sense some of these points have already been touched upon, others have been advocated in their reverse or positive forms. Incidentally these errors are not 'men of straw' but actually still lead to difficulties by way of interpretation of results from the systematic angle. It happens that the area of feeling and emotion is being investigated by research men with a wide difference in training and outlook. Clinicians, zoologists, psychologists, physiologists, endocrinologists, and many others have added their contributions and their results should be all the more valuable if they could be added together in the same column.

One cardinal confusion always to be avoided is the assumption that the description of the stimulus, the situation, or the series of events, completely or even approximately describes the feeling or emotion. Instance after instance could be cited from the literature to show that this criticism is not fictitiously or fancifully advanced. Undoubtedly the error arises from the customary experimental requirement that the physical conditions underlying any mental or bodily event should be carefully controlled and described. In regard to research on sensory discrimination and acuity, the relationship is of course so close that it can be stated in terms of psychophysical or psychometric laws. But in the field of feeling and emotion, as indeed in the case of all higher mental processes, where central neural conditioning or retroflex activities, to use Troland's terminology, have set in, this assumption is not only unsafe but misleading. The same physical conditions may produce very different types of feeling in different individuals or in the same individual from time to time. While it is rather unusual, I have known the threat of a gun and of an electric shock to be pleasant to a girl of college age and Jurgensen has recently proved that there are considerable variations in the assignment of hedonic values

to verbal associations by the same individual on different occasions. Even the evaluation of colors or odors by the impressive procedures depends ultimately on perceptual context. Therefore qualitative descriptions of the experiences involved in each case must be obtained from the observers.

Elsewhere I have already discussed the difficulty of leaning too heavily upon the bodily manifestations of feeling and emotion *per se*.¹⁴ Suffice it here to say, therefore, that while we need more accurate descriptions of bodily symptoms in all of their intricate manifestations under emotional and affective stimulation, we should not stop here. Often the investigator is so impressed with the discovery of some really significant physiological event which is well controlled that he throws most of his scientific precautions windward when he makes his psychological deductions. The great care which has been exercised in analyzing this bodily or more often neurological process frequently ends there and from that point on the psychological inferences proceed from a level in the central nervous system of the investigator no higher than the cervical region. The same investigator would never be caught saying that the generally anesthetized patient undergoing a major operation is suffering pain when he is groaning, but he sometimes will say that a decerebrate cat exhibits the experience of emotion of fear when its pattern of behavior suggests this feeling. I understand naturally that this sort of misconception arises from an ambiguous terminology, but scientific terms in the area of the affective life still require much repair. We should all be working as much in the direction of improving our concepts as we labor at actual experimental research. Descriptions of bodily processes and of grosser behavior patterns are not yet psychology: they must be related to mental processes to earn that title.

On the matter of unwarranted statistical embellishment of psychological data, we need now spend less time since

¹⁴ C. A. Ruckmick, The systematic position of emotion, *PSYCHOL. REV.*, 1936, 43, 417-426; Psychology tomorrow, *ibid.*, 1937, 44, 148-149.

Carr's extended and effective treatment of this subject has become available to our profession.¹⁵ As far as work in the emotional field is concerned, we have also to look to our foundations before we try to erase or to glorify the errors later on by statistical treatment. I agree with several of the psychometrists that sound experimentation is the first prerequisite to research and that elaborate personality schedules and paper tests of the affective life, even as regards such problems as motivation, can wait until more solid investigations give us more substantial facts. We used to watch the questionnaire procedure, now all eyes are on unnecessarily involved statistical treatments. I do not believe we should eschew statistical treatment; on the other hand controlled experimental situations come first and psychometrics second. Some of the 'constant errors' mentioned in preceding paragraphs may give fictitiously high correlations and yet the results may be psychologically meaningless. What is more pernicious, just because they have the approval of statistical treatment stamped upon them, they may be quoted for decades without questioning the ultimate source. In other words a very useful tool has definite applications; promiscuous use of that tool damages the work and partially destroys the future usefulness of that tool.

In the next place we who are working in the field of feeling and emotion have a right to hope that there will be increasing standardization of equipment and technique. Of course the galvanic apparatus has undergone rapid changes in the direction of improvement and of simplification of circuits. Adaptations to all sorts of purposes have been made. There is no use setting up an elaborate mechanism for photographic development in a machine where portability is the prime requirement. But in most cases the data obtained are a function of the method and the apparatus. For example, other things being equal, a scale arrangement that gives linear readings directly in ohms of resistance is preferred because it eliminates to a great extent recalibration and possible subjective errors, especially in view of the increasing

¹⁵ H. A. Carr, The search for certainty, *PSYCHOL. REV.*, 1937, 44, 274-296.

practice of perceptual interpretation, as we noted above. In the same way we have the problem of the treatment of the deflections. We are not yet sure that we know whether a percentage of change over the zero base-line of the balanced galvanometer is the best calculating technique. The experiences of many of the workers in the field again need pooling.

Verbal comments from the observers should also be scrutinized for scientific accuracy to see whether accepted psychological terminology can be applied. Better than that a certain amount of preliminary training in the process of observation, provided indoctrination can be reduced to a minimum, would be advisable. The field has grown up so rapidly that even the names of the most common emotions are as yet unstandardized. If so much importance attaches to the verbal report of the feeling and emotion as it is experienced, then it is highly desirable that all be done to make the information as reliable as possible at the source.

Much has been accomplished in the relatively short time that scientific investigators have been at work in the field of feeling and emotion. With the negative cautions presented as they are at the close of the paper, a serious disadvantage bears down on the true evaluation of important positive contributions toward a real and much needed complete understanding of the mind. But all is not plain sailing at the present time and we have much progress to make before we can be scientifically satisfied with the results. The work, however, is very encouraging and is sure to bring increasing satisfaction in a much neglected subject. As President Dykstra so well said in a recent address, the cardinal principles which govern man's progress in the world are intelligence, courage, and good-will. Of these he considered good-will the most important. By a not too loose translation we might substitute for these three, intellect, volitional action, and feeling, and then say that probably the most important of these three, all things considered, is feeling.

[MS. received October 4, 1937]

A REPLY TO PROFESSOR GUTHRIE

BY EDWARD CHACE TOLMAN

University of California

I find that I agree to a surprising extent with Guthrie's strictures on my theory of learning.¹ But I still do have a few difficulties.

First, for a couple of general ones:

(1) I think it is rather unfair for Guthrie to appropriate the term 'associative learning' and to identify it with his own particular brand of 'conditioning' and 'stimulus-response-ism.' I, also, like the term 'associative learning.' In fact, I should like to use it to cover the first five of my seven varieties of learning.²

(2) I do not see how his conditioned-response-ism will ever explain 'latent learning.' For latent learning provides a set-up in which the learning takes place even when the correct responses are not made appreciably oftener by the animal during the learning period itself than the incorrect responses. But I suspect that perhaps Guthrie does not really believe our supposed experimental 'facts' concerning latent learning.

But to come now, secondly, to the matter of the string-pulling situation itself.

(3) My feeling from watching the animals (this, of course, is pretty awful anthropomorphism) is that the first time they pull the string in, it is because they have already learned—i.e., that they have then and there just 'got,' at least tentatively, the required 'expectation' and that it is because they have got it that they then pull the string in—not that they pull the string in and then get the expectation.³

¹ E. R. Guthrie, Tolman on associative learning, *PSYCHOL. REV.*, 1937, 44, 525-528.

² E. C. Tolman, The acquisition of string-pulling by rats—conditioned response or sign-Gestalt?, *PSYCHOL. REV.*, 1937, 44, 203f.

³ Let me emphasize again and again that an 'expectation' does not require words nor consciousness—that it is just a 'set' for a certain environmental object-sequence.

(4) Guthrie is quite right in setting me straight that the 'conditioned-response' doctrine does not require only one preceding unconditioned stimulus and only one preceding unconditioned response for the conditioning to develop out of. I am grateful for the correction.

(5) The fact which Guthrie has emphasized here, as elsewhere, to wit, that a lot of stupid things get learned is, of course, extremely important. And, if most learning were of that sort, certainly the simple conditioning description would seem the more apt. As it is, however, there seems to be a great range from types of learning that look like pure 'dumb' conditioning at one end of the scale to ones which look like 'nice' and 'wise' expectations at the other.

And my feeling is that, if we must have but 'one principle' (and both Guthrie and I seem to be introverted enough to want one), it is going to be much easier to make 'dumb' conditioning a subordinate variety of expectation than to make expectation a subordinate variety of 'dumb' conditioning.

[MS. received November 15, 1937]

AN ECLECTIC VIEW OF SOME THEORIES OF LEARNING

BY W. N. KELLOGG

Indiana University

INTRODUCTION

To anyone who is interested in the explanation of learning, the various theories and interpretations which are current today present a bewildering picture. Many of the students of learning are aligned into separate groups or camps, each of which is intent upon furthering its own conceptions. So it is with some of the conditioned response adherents, who have pushed their doctrine to the point where it accounts for all learning, from habit-breaking to the development of elaborate skills. Among the Gestaltists, on the other hand, are those who would reduce conditioning to the development of insights. And between these views lies the trial-and-error hypothesis which seems to take little notice of the other schools. To the 'plain psychologist' who does not wish to take sides in the issue, the situation is certainly perplexing. If a single all-inclusive view of learning is correct, then competing views must certainly be wrong. Which of the theories shall he pick?

In the attempt to introduce clarity into this confusion, some authors have subdivided the field of learning into a few major categories of increasing difficulty. Different theories or interpretations of learning, so far as they fit the different categories proposed, are taken to apply to different kinds of learning. Thus Harlow has distinguished four major levels of learning, *viz.*, (1) non-voluntary or forced conditioning, (2) skills, (3) perceptual learning and (4) conceptual or symbolic learning.¹ Tolman in an earlier analysis has also listed four classes of learning, although they are not the same as

¹ H. F. Harlow, The neuro-physiological correlates of learning and intelligence, *Psychol. Bull.*, 1936, 33, 479-524.

those which Harlow lists.² More recently Tolman has expanded his list to seven.³

A second method which makes for unity in the field of learning is that employed by Dashiell.⁴ This author, who starts with the three primary divisions of trial-and-error learning, conditioning and Gestalt learning, has pointed to the common ground in each and has tried to show that real and basic distinctions do not exist. Such fundamental conditions as motivation, multiplicity of response, increase in general activity and selection or least action are common to each supposed kind of learning. It is Dashiell's point, therefore, that the discrepancies which appear between different interpretations have arisen from the bias and emphasis of particular theorists.

To the present writer, it would seem only reasonable to admit that any interpretation which has achieved the status of a well-known theory, cannot be wholly wrong or will ever be completely outmoded. The enormous amount of observation and experimentation, which backs up such views, assures a basis of scientific accuracy. Are we then to believe that each interpretation applies only to a special kind of learning, and that there are, as a consequence, as many discrete sorts of learning as there are theories to account for them? Or, are the principal theories of learning, as Dashiell has suggested, simply viewpoints of the same set of facts—viewpoints which are determined by the training and experience of the viewer? It is our purpose in this paper to examine four of the better-known theories of learning and to point out, if possible, some unrecognized relationships between them, so as to assist in a further clarification of the field. These theories are trial-and-error learning, Gestalt insight, conditioning and sign learning.

² E. C. Tolman, Theories of learning, in 'Comparative Psychology,' edited by F. A. Moss, New York: Prentice-Hall, 1934, pp. 367-408.

³ E. C. Tolman, The acquisition of string-pulling by rats—conditioned response or sign-gestalt?, *Psychol. Rev.*, 1937, 44, 195-211.

⁴ J. F. Dashiell, A survey and synthesis of learning theories, *Psychol. Bull.*, 1935, 32, 261-275.

LOW AND HIGH LEARNING

No one can deny that there are complex problems and simple problems in learning. For any given organism, some tasks are learned quickly and others are unsolvable. It is likewise true that certain of the theories of learning apply more readily to simple learning of a low or mechanical sort, and that other theories assume higher capacities on the part of the organism. A distinction of this kind has long been recognized, and is typified on the low or mechanical side by the terms 'motor,' 'muscular,' and 'physiological' learning; on the side of higher learning, by the terms 'ideational learning,' 'inferential learning,' and 'symbolic learning.'

If learning is divisible into simple and complex varieties, then it would appear that Gestalt learning must belong in the latter group. There is certainly an affinity between symbolic learning and learning of the Gestalt type. For one thing, the organism is responding to more than one stimulus in each case. It makes a kind of relational reaction, the success of which depends upon several factors. Some of the stimuli may be kinesthetic or symbolic. But this does not alter the fact that what may be called the 'field of stimulation,' is, in each case, broad. Again, each view seems willing to attribute to the organism an ability beyond that which is directly observable through its behavior. Obviously, they are liberal interpretations. They give the animal the benefit of the doubt. It is a question whether symbolic or Gestalt learning should be described at all in purely mechanistic terms.

Conditioned response learning and trial-and-error learning, on the other hand, are simple types of learning, not necessarily because, as Tolman has put it, each is based upon neural or upon other sorts of 'connections,' but rather because they are fundamentally objective and automatic. They are mechanistic interpretations freed for the most part from symbolism or 'ideas.' The learning in a sense takes place itself, providing the requisite conditions for learning are satisfied. The learning organism assumes a kind of passive part in the learning situation. What he does seems to a greater extent to be

forced upon him, and less a contribution of his own, than is the case in higher learning.

TRIAL-AND-ERROR LEARNING

Having raised a distinction of this sort on traditional and logical grounds, let us now ask whether the distinction can be bridged, and whether there is really any fundamental qualitative difference between 'low' learning and 'high.' The answers to these questions can be discovered most easily by means of a series of diagrams, in terms of which the four theories of learning to be discussed can be more clearly presented. Let us take first the trial-and-error view as proposed by Thorndike and as described by numerous writers today. Trial-and-error learning is essentially a process of selecting one response from among many, the selection taking place as a result of the operation of the Law of Exercise, the Law of Effect, and more recently the Law of Belonging.⁵ In order to show the progress of learning over a period of time, let us break up the process into three phases or steps. The most elementary or initial stage would represent the situation as it exists at the start or beginning of the learning process. The second or intermediate stage then represents the situation as the learning is taking place, and the final or completed stage would depict the situation after the learning has been accomplished.

In Fig. 1, these three stages are diagramed as they exist according to the trial-and-error hypothesis. *S* is the stimulus situation (maze, problem box, puzzle, etc.) and *R*1 . . . *R*7 are incorrect responses or errors. *RC* is the correct response leading to a solution of the problem at the termination of any given presentation of the stimulus. In Stage 1 (Fig. 1) the conditions are those which exist at the start of the learning process. A great many errors are made (and often the same error is repeated) before the correct response, *RC*, occurs. Progress in learning is shown in Stage 2 where the number of errors is considerably reduced, but not entirely eliminated.

⁵ A detailed consideration of these principles is omitted entirely from the present discussion, which seeks only to examine the theory as it exists.

In Stage 3, the learning is complete. Any presentation of *S* now calls forth *RC* immediately, without the occurrence of any other *R*'s at all. The gradual decrease in the number of errors, if recorded at each presentation of the stimulus situation, gives the 'error' learning curve typical of situations of this sort.

If learning is now regarded as the gradual development of an insight into the nature of the problem, as Wheeler⁶ has suggested, then there is no point in distinguishing between trial and error learning and learning by insight. Trial-and-error observations become simply an objective record of the

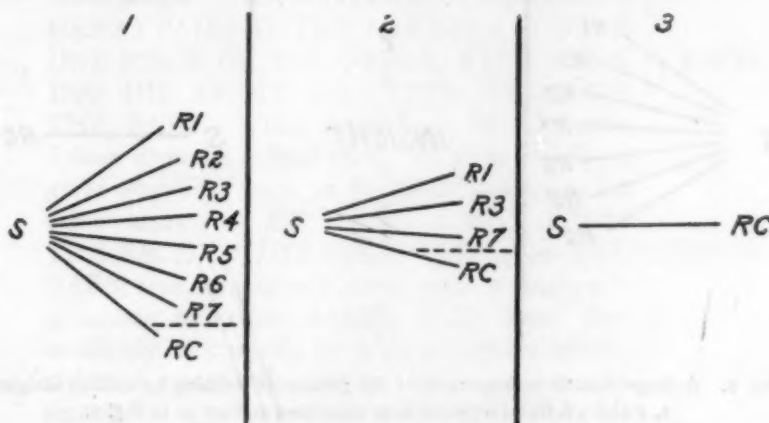


FIG. 1. The trial-and-error theory in diagram form. 1, 2, and 3 represent successive time periods in the learning. Step 1 depicts the process at the beginning of a series of trials. Step 2 represents a 'sample' trial in the middle of the learning process. And Step 3 shows the final stage when the learning is complete.

development of 'an insight.' Conversely, learning by insight becomes the subjective or introspective aspect of trial-and-error learning. They are one and the same activity conceived (1) from the point of view of the experimenter and (2) from the point of view of the subject.

SUDDEN INSIGHTS AND TRIAL-AND-ERROR

But if the insight comes suddenly, according to the traditional method described by Köhler in his *Umweg* experi-

⁶ R. H. Wheeler, *The science of psychology*, New York: Crowell, 1929, p. 252.

ments,⁷ then it may at first appear that there can be no similarity between trial-and-error learning and Gestalt learning. Yet it is our contention that such a similarity exists, and that the two are only variants of one and the same process except that trial-and-error learning is the more elementary version ('low' learning) and learning by sudden insight is the more complex version ('high' learning). The diagrams in Fig. 2 may serve to make this clear.

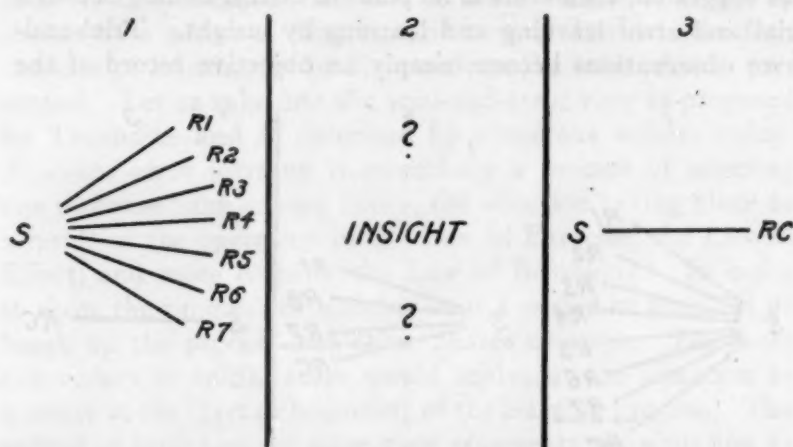


FIG. 2. A diagrammatic arrangement of the process of learning by sudden insight. 1, 2 and 3 refer to separate time sequences as they do in Fig. 1.

Unless the solution of the problem is *immediately apparent* to the subject so that no false attempts (errors) are made at all, the condition which exists at the beginning of the learning is as indicated in Stage 1, Fig. 2. *The subject does make errors*, despite popular misconceptions to the contrary, providing the problem is difficult enough to require any new adjustments at all and cannot be 'seen into' as soon as it is presented.⁸ The classical description by Köhler of the solution of the jointed stick problem by the chimpanzee, Sultan, may be taken as an illustration of this point. The statements which are capi-

⁷ W. Köhler, *The mentality of apes*, 2nd ed., New York: Harcourt-Brace, 1927, pp. 336.

⁸ In such cases, it may be argued there is no real problem of *learning* at all, and hence the situation is not a typical learning situation.

talized in the quotation below describe successive trials and errors.⁹

Are the two sticks ever combined so as to become technically useful? This time Sultan is the subject of experiment. His sticks are two hollow, but firm, bamboo rods, such as the animals often use for pulling along fruit. The one is so much smaller than the other, that it can be pushed in at either end quite easily. Beyond the bars lies the objective, just so far away that the animal cannot reach it with either rod. They are about the same length. **NEVERTHELESS, HE TAKES GREAT PAINS TO TRY TO REACH IT WITH ONE STICK OR THE OTHER, EVEN PUSHING HIS RIGHT SHOULDER THROUGH THE BARS.** When everything proves futile, Sultan commits a 'bad error,' or, more clearly, a great stupidity, such as he made sometimes on other occasions. **HE PULLS A BOX FROM THE BACK OF THE ROOM TOWARDS THE BARS;** true, he pushes it away again at once as it is useless, or rather, actually in the way. Immediately afterwards, he tries something which, although practically useless, must be counted among the 'good errors'; **HE PUSHES ONE OF THE STICKS OUT AS FAR AS IT WILL GO, THEN TAKES THE SECOND, AND WITH IT POKES THE FIRST ONE CAUTIOUSLY TOWARDS THE OBJECTIVE,** pushing it carefully from the nearer end and thus slowly urging it towards the fruit. This does not always succeed, but if he has got pretty close in this way, he takes even greater precaution; **HE PUSHES VERY GENTLY, WATCHES THE MOVEMENTS OF THE STICK THAT IS LYING ON THE GROUND, AND ACTUALLY TOUCHES THE OBJECTIVE WITH ITS TIP.** Thus, all of a sudden, for the first time, the contact 'animal-objective' has been established. . . .

ERROR

ERROR

ERROR

ERROR

⁹ Capitalizing is mine.

THE PROCEEDING IS REPEATED; WHEN THE ANIMAL HAS PUSHED THE STICK ON THE GROUND SO FAR OUT THAT HE CANNOT POSSIBLY GET IT BACK BY HIMSELF, IT IS GIVEN BACK TO HIM. . . . SULTAN, AS BEFORE, PUSHES ONE STICK WITH THE OTHER TOWARDS THE OBJECTIVE, and as this pseudo-solution does not satisfy him any longer, he abandons his efforts altogether, and does not even pick up the sticks when they are both thrown through the bars to him. The experiment has lasted over an hour, and is stopped for the present. . . . As we intend to take it up again after a while, Sultan is left in possession of his sticks; the keeper is left there to watch him.

ERRORS

Keeper's report: Sultan first of all squats indifferently on the box, which has been left standing a little back from the railings; then he gets up, picks up the two sticks, sits down again on the box and PLAYS CARELESSLY WITH THEM. While doing this, it happens that he finds himself holding one rod in either hand in such a way that they lie in a straight line; he pushes the thinner one a little way into the opening of the thicker, jumps up and is already on the run towards the railings, to which he has up to now half turned his back, and begins to draw a banana towards him with the double stick. I call the master: meanwhile, one of the animal's rods has fallen out of the other, as he has pushed one of them only a little way into the other; whereupon he connects them again.¹⁰

ERRORS?

What Sultan did (and what any organism does in learning of this sort), *up to but not including the final successful response*, is schematically represented in the first stage of Fig. 2. The final response is indicated in Stage 3. It would also be possible to diagram the correct solution, *RC*, at the bottom of the series of responses in Stage 1, and still obtain a true and

¹⁰ Köhler, *op. cit.*, 125-127.

accurate picture of the situation. The arrangement shown in Fig. 2 was chosen because it can be more easily compared with the diagrams in Fig. 1. The essential fact here is that, as usually recorded, there is *not* a gradual reduction in the number of errors, from Stage 1 (before the learning has taken place) to the final stage of complete learning. Step 2 of the trial-and-error process (2, Fig. 1) is missing in the present instance and a 'sudden insight' is substituted for it. The insight, however, has the same effect as a long and gradual elimination of errors, in that it places the learner at the same point of efficiency he would have reached at the culmination of a regular trial-and-error process. In each instance, after the learning has been completed, succeeding presentations of the stimulus lead to the immediate calling out of *RC*. Trial-and-error learning and learning by sudden insight are similar then as to the initial and final steps, but differ chiefly in the intermediate one. The principal distinctions may be summarized as follows:

<i>T and E</i>	<i>Sudden Insight</i>
Gradual elimination of errors	Abrupt elimination of errors
The learning is not complete upon the first occurrence of <i>RC</i>	After <i>RC</i> has once been insightfully made, it is always thereafter called out upon subsequent presentations of <i>S</i>
Time of learning usually long (Gradual building up of insight?)	Time of learning shorter Sudden insight

To what are these differences ascribable? It is certainly not to a difference in organisms, since a gradual elimination of errors has been shown to exist over almost the whole phyletic scale, and since learning by insight has been demonstrated even in chickens and rats.¹¹ The same organism may at different times learn by trial-and-error and learn by insight. Are we then to suppose that the organism itself possesses a

¹¹ Köhler, *op. cit.*, p. 14; H. Helson, Insight in the white rat, *J. Exper. Psychol.*, 1927, 10, 378-397; G. D. Higginson, Visual perception in the white rat, *J. Exper. Psychol.*, 1926, 9, 337-347.

kind of reservoir of learning techniques, from which it selects this, that or the other as the occasion demands?

There is one obvious source to which the differences listed above are traceable, and when this source is taken fully into account, the differences become simply differences in degree rather than in kind. They become quantitative rather than qualitative differences. That source, as has been previously pointed out, is the fundamental difference in the degree of difficulty of the problems which are solvable by the two methods. A very difficult learning problem which cannot be solved at once, leads to a gradual elimination of errors and so to trial-and-error learning. A problem well within the grasp of the organism may be solved immediately. There need therefore be no sharp rift between these two interpretations since each is reducible to the other if the difficulty of the problems involved is considered. Learning of the sudden insight variety is equivalent to trial-and-error learning that occurs upon a single presentation of the stimulus situation. The final stage of learning is reached after a single correct insightful response, as demonstrated by the fact that subsequent presentations of the problem situation lead at once to correct solutions. This means that the problem must have been simple enough for the organism to retain completely the methods used in its solution, after having solved it but a single time. When such a retention is not possible because the difficulty of the problem prevents the immediate formation of a Gestalt, then—motivation being maintained for successive presentations of *S*—some of the errors originally made in Stage 1 are bound to be repeated before the learning is complete. If a difficult task, which would ordinarily require a trial-and-error solution, can be decreased in difficulty, learning by sudden insight may result. The apparent difference between the two varieties of learning arises, therefore, simply from the great difference in the difficulty of the learning situations habitually employed in each case. To obtain evidence of trial-and-error learning, use difficult learning problems. To get sudden insights in the same organism, use relatively simple problems. Where insight is clearly a special case of positive transfer, which is undoubtedly true in

some instances, then a second difficult problem, presented after a former similar one has been mastered by trial-and-error, may demonstrate insight. The second problem in such instances has lost its difficulty because of the previous learning the subject is able to bring to bear upon its solution.

Cases of learning, like those diagramed in Figs. 1 and 2, are best conceived as extreme examples. Between them we would suppose there may exist almost any combination or degree of the two. Instances in which sudden drops occur in otherwise orthodox learning curves are a case in point, as are, perhaps, some situations in which the appearance of the insight is not immediate but develops by more easy stages. We would suppose, therefore, a sort of scale or continuum, of which 'pure' trial-and-error learning and Gestalt sudden insights are the poles. The intermediate degrees would be determined in the main by the relative difficulty of the problems employed.

CONDITIONING

Let us now turn to conditioning and its possible relationship to the other types.¹² It will be remembered that the writer has already suggested that conditioning and trial-and-error learning can, with some justification, be called 'low' interpretations of learning in the sense that they are generally supposed not to involve as many of the higher mental or psychic processes as are assumed in symbolic and insightful learning. Learning of the conditioned response variety, although customarily diagramed without reference to its temporal aspects, is shown in Fig. 3, divided into three progressive steps. Step 1 (Fig. 3) may serve, as in Figs. 1 and 2, to represent the situation before learning has taken place. Step 3 shows the situation after the learning is complete, and Step 2 gives a picture of the intermediate state.

It should be noted that the S's in this diagram do not carry quite the same significance as the S's in Fig. 1 or Fig. 2. In Fig. 3, S1 and S2 stand for specific features of the general

¹² For a detailed analysis of this question in its experimental aspects, see E. R. Hilgard, The relationship between the conditioned response and conventional learning experiments, *Psychol. Bull.*, 1937, 34, 61-102.

situation commonly analyzed or conceived as elementary or unitary stimuli. In Figs. 1 and 2, on the other hand, *S* was used to represent the whole stimulus situation, or field, which included many elementary stimuli.

Stage 1 (Fig. 3) shows that a given functional unity must exist between a stimulus and a response before conditioning can begin. In traditional Pavlovian terms, *S*₁ is the unconditioned stimulus and *R* is its response. The learning process itself is characterized by the introduction of *S*₂ which may or may not possess a readily observable *R* itself. Step 2 in the diagram then represents the many repetitions of *S*₁,

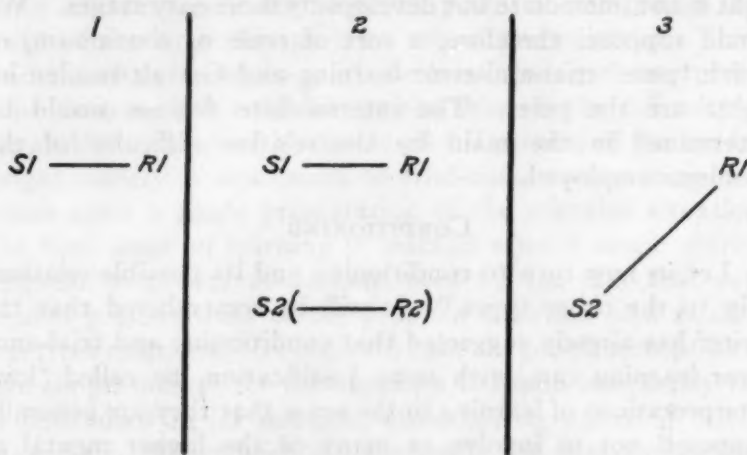


FIG. 3. Showing conditioned-response learning, after Guthrie, diagramed in three stages so as to show the progress of the learning (see text).

*S*₂, and *R*₁ which are necessary for the completion of the conditioning process. *S*₂ has been drawn immediately below *S*₁ to suggest a simultaneous temporal order of *S*'s and *R*'s. The arrangement pictured in Step 2 may, therefore, be taken to follow the position of Guthrie who has held that all conditioning is simultaneous conditioning, if the true stimuli and responses can be adequately discovered.¹³ In Step 3, which

¹³ E. R. Guthrie, *The psychology of learning*, New York: Harpers, 1935. It is Guthrie's view that the response conditioned must occur simultaneously with the stimulus which later calls it out. The diagram in Step 2, Fig. 3 does not fully picture this arrangement unless (1) the *R*'s are imagined to extend back to the *S*'s or (2) unless the lines between the *S*'s and the *R*'s are taken to represent the passing of no more time than the reaction time of the organism.

represents the situation after the learning has been completed, S₂ calls out R₁ with S₁ no longer present.

Fig. 3 as a whole covers the elementary types of conditioned reflexes studied by Pavlov and his followers. It needs no insightful or symbolic assumptions but serves as a simple description of this mechanical type of low or physiological learning.

SIGN LEARNING

Probably the most direct connection that can be traced between elementary conditioned response processes of this sort and more psychic or higher varieties of learning lies in the direction of Tolman's sign-Gestalt theory. According to Tolman, learning is essentially a matter of discovering and retaining 'what leads to what.'¹⁴ It is a matter of reacting to certain stimuli, which always come before other stimuli, as signs or signals that the other or secondary stimuli are about to arrive. Learning reduces itself to anticipation of a sort and the conditioned response itself is explained by Tolman as definitely anticipatory. The learner forms new Gestalts or perceptual patterns by bringing the new stimulus into relationship with the old when it was not originally a part of the same Gestalt. Tolman's view has the advantage of explaining those cases of conditioning in which it has been definitely established that the conditioned response is not the same as the unconditioned one.¹⁵ It also applies nicely to instances in which the conditioned stimulus invariably precedes the unconditioned stimulus ('forward' conditioning).

¹⁴ E. C. Tolman, *Purposive behavior in animals and men*, New York: Century, 1932; *Theories of learning*, in 'Comparative Psychology,' edited by F. A. Moss, New York: Prentice Hall, 1934, pp. 367-408.

¹⁵ Cf., e.g., C. R. Garvey, A study of conditioned respiratory changes, *J. Exper. Psychol.*, 1933, 16, 471-503; E. R. Hilgard, and D. G. Marquis, Acquisition, extinction, and retention of conditioned lid responses to light in dogs, *J. Comp. Psychol.*, 1935, 19, 29-58; E. R. Hilgard, and A. A. Campbell, The course of acquisition and retention of conditioned eyelid responses in man, *J. Exper. Psychol.*, 1936, 19, 227-247; H. D. Scott, Hypnosis and the conditioned reflex, *J. Genet. Psychol.*, 1930, 4, 113-130; L. H. Warner, An experimental search for the 'conditioned response,' *J. Genet. Psychol.*, 1932, 41, 91-115; G. R. Wendt, An analytic study of the conditioned knee-jerk, *Arch. Psychol.*, 1930, No. 123, p. 97; E. G. Wever, The upper limit of hearing in the cat, *J. Comp. Psychol.*, 1930, 10, 221-234.

A diagram of sign-Gestalt learning, modified from Tolman so as to follow the three-step arrangement employed in this paper, is given in Fig. 4.¹⁶ Here, as in Fig. 3, S_1-R_1 represents the situation which exists before any learning takes place. S_2 is the new (conditioned) stimulus which may or may not have a clear-cut response of its own. It is introduced during the learning, so that it invariably occurs prior to S_1 .¹⁷ When the learning is complete (Step. 3, Fig. 4), S_2 calls out a new response, R_N , which was formerly not associated either with S_2 or with S_1 . S_2 has then become a sign or signal to S_1 . In responding to S_2 with R_N the organism may be said

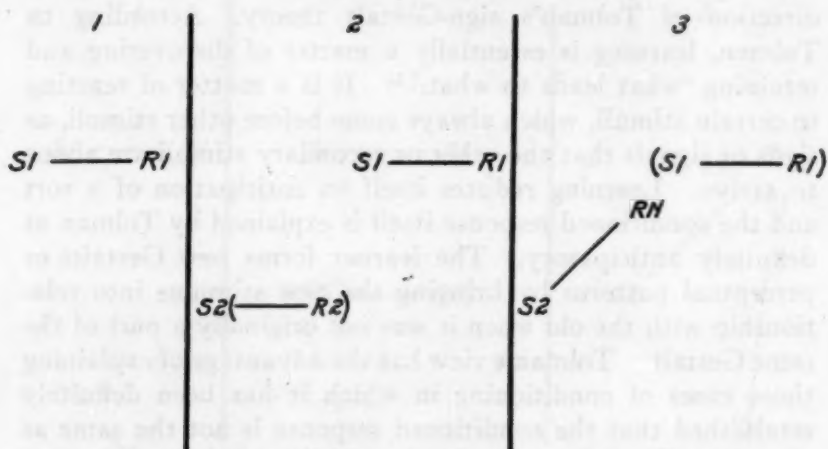


FIG. 4. Sign learning, diagramed in three steps comparable to those in Fig. 3.

to demonstrate that 'it knows' S_1 is coming. It makes, in other words, an anticipatory or preparatory reaction by means of which it gets ready for S_1 . R_N will thus appear after S_2 , even though S_1-R_1 is not included in the situation. This follows from the fact that S_2-R_N always precedes S_1-R_1 . The non-experimental example given below, chosen from among those suggested by Tolman, will show the operation of this sequence in terms of Fig. 4.

¹⁶ E. C. Tolman, *Theories of learning*, in 'Comparative Psychology,' edited by F. A. Moss, New York: Prentice-Hall, 1934, pp. 367-408.

¹⁷ S_2 and R_N have been drawn to the left of S_1 to indicate that they occur before S_1 .

. . . My dog has come to react to the noise of my returning automobile [S₂] before she can see the machine [S₁] and I find her waiting for me on the sidewalk [RN] when I and the car finally loom into sight. She has learned to respond to the sound [S₂] as a "sign" for the car and me in it [S₁]. Further the response she has thus attached to the sign is not the same as the response she makes to the car when I finally arrive [R₁]. The response [RN] to the sign [S₂] is a preparatory coming forward, that to the actual car with me in it, one of scrabbling up and wet kisses [R₁].¹⁸

The close similarity between the formation of a new Gestalt of this character (*i.e.*, between the dog's getting insight into the meaning of the sound of the automobile) and the lower or more elementary conditioned reflex type of learning should now appear from a comparison of Figs. 3 and 4. If Fig. 3 represents conditioning on a physiological level, then Fig. 4 may be said to represent conditioning on a psychological level. The principle objective differences between the two processes appear to be (1) the setting of S₂ (—R₂) invariably in advance of S₁—R₁, and (2) the introduction of a new response, RN, in place of the formation of a connection between S₂ and R₁.

It would be our view that 'low' conditioning, as diagrammed in Fig. 3, is one extreme of this pattern of learning and that sign-learning, as diagrammed in Fig. 4, is a higher aspect of the same general process. In the language of Guthrie,¹⁹ who has pointed to the common ground between conditioning and association by contiguity, we may say that there are here two varieties of association—one elementary and the other more complex. Or, to use Gestalt terminology, we have in each case a new configuration of elements (stimuli and responses) not originally Gestalted. The present analysis assumes a sort of continuum from simple to complex instances of conditioned-learning. Between the two examples discussed we would suppose there lies a range of intermediate possibilities. The

¹⁸ E. C. Tolman, Theories of learning, in 'Comparative Psychology,' edited by F. A. Moss, New York: Prentice-Hall, 1934, pp. 368-369.

¹⁹ E. R. Guthrie, The psychology of learning, New York: Harpers, 1935, p. 23.

relationship between conditioning and sign learning is similar then, as we conceive it, to that already outlined between trial-and-error and insightful learning.

A SINGLE PRINCIPLE OF LEARNING

The step which would logically follow in a simplification of this sort would now be to bring together the two groups of theories which have been pointed out. There are several ways in which they could be united under the heading of a single general principle. Any of the ways would be consistent with the present argument.

1. We could, for example, follow the methods of Frank,²⁰ Hull,²¹ Smith and Guthrie,²² Wilson²³ and others, and reduce trial-and-error learning, as demonstrated in the maze and problem box, to conditioning. Since conditioning, according to our reasoning, includes sign learning, and since trial-and-error is but an extreme variety of Gestalt, all would become particular cases of the conditioned response.

2. Or, to start from the Gestalt point of view, we might use the system of Tolman²⁴ and explain trial-and-error learning as the building up of discriminatory sign-Gestalts.

3. It would also be possible to follow more exactly the deductions of the present paper, and combine these with the fundamental geometrical theorem, "Things equal to the same or equal things are equal to each other." Since it has already been shown that trial-and-error learning is a variety of Gestalt (and *vice versa*), and that conditioning is but an aspect of sign-Gestalt learning, we could then say that conditioning

²⁰ J. K. Frank, Suggestion for a theory of learning, *PSYCHOL. REV.*, 1923, 30, 145-148.

²¹ C. L. Hull, A functional interpretation of the conditioned reflex, *PSYCHOL. REV.*, 1929, 36, 498-511; Simple trial and error learning—a study in psychological theory, *PSYCHOL. REV.*, 1930, 37, 241-256.

²² S. Smith, and E. R. Guthrie, General psychology in terms of behavior, New York: Appleton, 1921, p. 124f.

²³ W. R. Wilson, Principles of selection in 'trial and error' learning, *PSYCHOL. REV.*, 1929, 36, 481-487.

²⁴ E. C. Tolman, Purposive behavior in animals and men, New York: Century, 1932, pp. 339-370.

and trial-and-error are equivalent, since they are both reducible to a common or equivalent base.

It is doubtful whether much would be gained from such procedures, or whether they would serve to give complete, yet unstrained, interpretations of the facts. There are certain special characteristics of conditioning which appear to us at the present writing to make a separate classification of it, of some convenience. Learning of the conditioning, or association type, is always more simple and more easily broken up into units than trial-and-error learning. There is often no 'problem' to 'solve' in the customary sense of the word as there is in trial-and-error and other kinds of learning. Again, conditioning and sign-learning, like association, are in a sense *positive processes*, comparable to the process of addition. Something is added, or hooked on, or connected, to something else. Although the additive process can also be seen in trial-and-error learning, it is not so clear or prominent as the subtractive process or the elimination of errors. Conditioning, therefore, might almost be called, 'Learning by Addition,' and trial-and-error, which is a kind of *negative process*, 'Learning by Subtraction.'

That such a distinction cannot be carried too far, however, is apparent when it is considered that both of these processes go on in any complicated learning situation. There is not only an elimination of wrong activities, but there is also an introduction of new responses which were not originally called out by the stimuli or objects of the situation. The individual has to learn *what not to do* as well as *what to do*. He is frequently learning both at the same time.

It can be said, in fact, of the different theories, that each has taken some part or feature of the learning situation and has emphasized it to the exclusion of other parts. Trial and error learning emphasizes the wrong responses which are made at the beginning of a gradual learning process. Conditioning emphasizes stimulus-response units and the principle of association. The Gestaltists make much of the introspective or psychic side of learning, or sometimes of the suddenness of the transition from wrong to right responses.

And Tolman has pointed to the temporal sequence of stimuli and responses. He has emphasized the time element in learning.

The relationships between the four theories discussed, together with special characteristics applying to each of them, have been diagrammed in Fig. 5. The top row in columns 2 and 3 lists a few of the objective features of the learning which stand out, while column 1 gives some psychological and

SCHEMA SHOWING RELATIONSHIPS BETWEEN FOUR THEORIES OF LEARNING

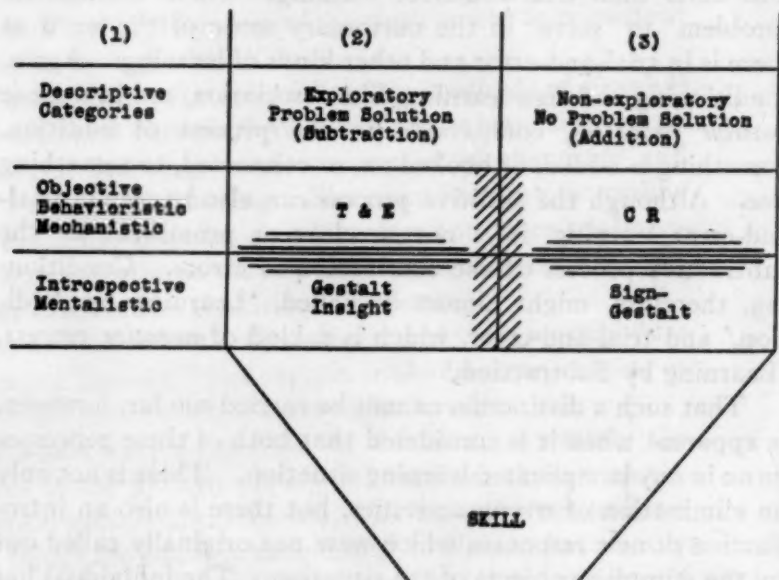


FIG. 5. Some similarities and differences between the trial-and-error hypothesis, conditioning, sign learning and gestalt insight are represented in this diagram. See text for meaning of shaded and angular lines.

descriptive terms applying to the views indicated. Continua must be imagined to exist in columns 2 and 3 between the upper and lower entries. The vertical line separating these two columns is a hazy one to indicate that the boundary is not hard and fast. It is a boundary which can easily be bridged. Difficult or elaborate problems, in actual practice, may be said to include learning which is describable in terms of any, or of all, of the theories.

SKILLS

There has been up to the present no mention in this paper of the acquisition of complex skills such as mastery in typing, in speaking a foreign language, or in playing tennis. Is it necessary to invent an entirely new hypothesis to account for this sort of development? We think not. The development of a skill can be explained as the learning which goes on after or beyond the sorts of learning which have already been discussed. The perfection of a skill has nothing to do either with 'addition' or 'subtraction' in learning. It is not concerned with the beginning of a new response or series of responses, or with the elimination of old responses. Rather is it concerned with the giving of a certain fluency, speed and precision to complete reactions after their nature has already been determined. Improvement in skill is not a matter of finding out what is to be done; but of doing it better and better. Skills may, therefore, be placed under the heading of over-learning. Their single most important principle is the Law of Exercise, upon which their efficiency must ultimately depend. They start from a point where the theories already considered end and they move forward from that point. An attempt has been made to indicate this relationship, in the schema of Fig. 5, where skills have been placed beyond and apart from descriptions of the original learning.

SUMMARY

Some of the proponents of different theories of learning have so zealously defended their positions that psychologists who are not committed to one view or another may find the field a complex and confusing jumble. It has been the aim of this paper to assist in clarifying the situation. To this end the trial-and-error hypothesis, insightful learning, conditioning, and sign learning have been separately discussed and analyzed. Each theory has been reduced, for purposes of simplification, to a series of three-stage diagrams. Examination of these diagrams has shown:

1. A close correspondence between trial-and-error learning

and learning by insight. A continuum has been proposed between these two extremes.

2. A further correspondence between conditioning, as described by Guthrie, and sign learning, after Tolman. These also have been viewed as varieties of a common process.

3. It has been pointed out that each particular theory has served to emphasize some special feature of the learning situation to the exclusion of other features.

4. The writer sees no point in trying to reduce all learning to a single general principle, but holds that it may be of some convenience to classify learning in which the elimination or reduction of responses is emphasized (as in trial-and-error learning) separately from learning in which new stimulus—response relationships are emphasized (like conditioning). Both go on, however, in any complex learning situation.

5. In difficult or involved learning, therefore, activities which can be adequately accounted for in terms of any, or of all, of the learning theories take place.

6. It is proposed that the term 'skill' be reserved for the development of fluency and precision of response, *after the right reactions have been integrated*. Skill is then a matter of over-learning, which is apart from, and beyond, the learning theories discussed.

[MS. received July 20, 1937]

THE SYNTHESIS OF INTELLIGENCE—ITS IMPLICATIONS

BY THOMAS ROSS

University of Washington

Whosoever believes that 'learning,' 'intelligence,' 'mind' are merely convenient terms for designating the workings of the ordinary laws of nature through special structures must show just how functions that are properly described by these terms can spring from the functionings of structures that are well understood. Unfortunately, the structures that sustain human and animal intelligence are not well understood. Pending the hoped-for ultimate success of physiologists in finding out about these matters, psychology can hardly spare the use of whatever means may come to hand for clearing its theoretical atmosphere.

One way to be relatively sure of understanding a mechanism is to make that mechanism. To find the sufficient conditions for learning we should try to make a machine that will learn. This has been done several times in the past few years, and has perhaps been somewhat overdramatized.

A very persistent mistaken attitude toward work of this sort is the idea that the builder of a machine which will learn must think he has built a mechanism physically like that underlying human or animal learning. Nothing could be further from the truth. What is demonstrated by the physical existence of a performing machine is that a machine is capable of that kind of performance. It is not demonstrated, however, that only this sort of machine can produce the given effects; for no truth is more commonplace in mechanics than that, in general, several alternative mechanisms, differing widely in superficial characteristics and forms of energy utilized, can produce the same end-result.

What seem to be the irreducible requirements for any

mechanism or organism that is to learn a serial response without receiving successive differential cues from the outside are shown by a machine completed in the summer of 1935.¹ Some of the handicraft and certain of the mechanical principles were furnished by the writer, most of the actual construction having been done by W. A. Dillman and Byron Sullivan, the departmental mechanics.

This machine runs through a twelve-unit, multiple-Y maze in which the twelve sections are so arranged that each has one blocked branch and one branch that opens into the stem of the next section. Placed at the beginning of this maze and set in motion by connection to an electrical supply, the machine will begin rolling through the maze on its three wheels. Being constructed with a tendency to turn to the right, it will, on coming to the forking of the first Y-section, run down the right-hand passage. If this passage is blocked (by a vertical wall at the end) the machine will back out of that passage and turn to the left upon again starting forward. Since the maze is so arranged that one passage in each section is open, the left-hand passage will now lead directly to the beginning of the next section of the maze, at which point the machine will receive a 'cue' that it has entered a new section as it brushes its two metal 'feelers' against a pair of blocks set on the sides of the passage. In this next section of the maze the machine will again take the right hand passage at first, simply passing on through if possible but backing and turning to the left if encountering an obstacle, and so through the whole twelve unit maze. On entering each new section, of course, it will pass between another pair of blocks, but nothing will be registered in the mechanism to enable it to discriminate one pair of blocks from another as such. The blocks are all the same.

On being again started through the maze, the machine will go from beginning to end without entering any of the blind passages which it entered on the first trip through, and every time thereafter will repeat the performance without

¹ This machine was made at the University of Washington under the direction of Dr. Stevenson Smith.

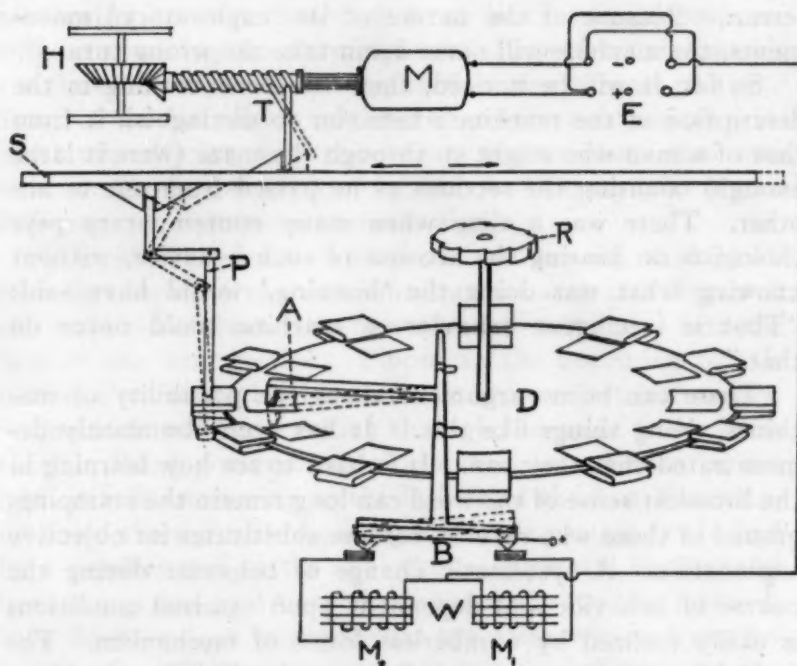
'error.' Because of the nature of its 'exploratory' movements, the machine will never again take the wrong turn.

So far, it will be noticed, there has been nothing in the description of the machine's behavior to distinguish it from that of a man who might go through the maze (were it large enough) counting the sections as he passed from one to another. There was a time when many contemporary psychologists on hearing the account of such behavior, without knowing what was doing the 'learning,' would have said: "That is intelligent behavior—a machine could never do that."

There can be no argument about the possibility of machines' doing things like this. It has been abundantly demonstrated that they can. It is hard to see how learning in the broadest sense of the word can long remain the stamping-ground of those who revel in verbose substitutes for objective explanation. A systematic change of behavior during the course of behavior and dependent upon external conditions is easily realized by numberless forms of mechanism. The fact that maze learning can be a mechanical function in a man-made tricycle should give us added confidence that the physical mechanism of the more elaborate forms of learning in animals can be discovered. A machine is sufficient, even if not necessary, for the task of learning.

The particular mechanism used in the maze learning machine, the behavior of which has been described, is shown in highly schematized form in the accompanying diagram. Proportions and positions of parts have been liberally distorted in order to attain simplicity, and to the same end those parts which are not necessary for an understanding of the functioning of the mechanism are not represented in the diagram.

It will be seen from the diagram and accompanying description that the ability of the machine to learn the maze is due to the presence of the part called the "memory disk" upon which a pattern of raised and depressed tabs corresponding to the pattern of right and left turns needed for traversal of the maze without backing is impressed.



W marks the position occupied by a piece of iron the movements of which control the set of the pilot wheel of the maze learning machine. M_1 and M_2 are electromagnets acting upon the piece of iron. *B* is a metal contact bar carrying points which control the current to the electromagnets. The alternative positions of *B*, shown solid and dotted, determine which magnet shall receive current, and are determined by the positions of the arm *A*, which is rigidly attached to the same shaft. The position of *A* is determined by whether or not the hole above which its end is centered at any particular time is open or is stopped from below by one of the brass strips the ends of which protrude from the edge of the memory disk, *D*. These brass strips, when the machine is first placed in the maze, are all set so that their ends hook over the edge of the disk, *D*, thus holding them in the elevated position in which most of them are shown. The action of the plunger, *P*, is to unhook the end of the strip below it from the edge of the disk, thus allowing it to drop to the position shown by the dotted outline. The plunger descends only when, in the forward movement of the machine, the end of the rod *S* is brought by collision against one of the barriers marking the closed end of a maze passage and transmits part of the force of the impact to the plunger through the lever system shown. Tracing this through, it will be clear that the machine will tend to turn left only after it has struck a barrier, M_1 being the magnet that swings the pilot wheel to the right, and M_2 the magnet swinging the wheel to the left.

The ratchet wheel, *R*, is part of the mechanism (the rest not shown) which swings the memory disk through one-twelfth of a revolution every time the machine passes between the paired blocks that mark the beginning of a new maze section. Other mechanisms are the reversing switch *E* which is thrown when the rod *S* is driven backward by collision with a barrier; the motor *M* which is reversed; the hind wheel assembly *H* which is driven by the motor; and the worm and toggle system *T* which acts to restore the longitudinal rod to the forward position after the machine has backed a certain distance. (Pitch of worm greatly exaggerated.)

The completely abstracted principle of serial learning, which is all that the machine illustrates, is that in order for a serial act to come to be elicitable by a series of exteroceptive cues which do not differ from one another, there must at first be a pattern of original cues (in this case, collision with barriers) effective in eliciting each response of the series in turn, and corresponding to this pattern of differential cues must be some pattern effect produced within the learning machine or organism. Upon such an internal effect the reproduction of any behavior pattern without differential cues from the outside must depend.

[MS. received July 7, 1937]

PSYCHOLOGICAL REVIEW PUBLICATIONS

Original contributions and discussions intended for the Psychological Review should be addressed to

Professor Herbert S. Langfeld, Editor PSYCHOLOGICAL REVIEW,
Princeton University, Princeton, N. J.

Original contributions and discussions intended for the Journal of Experimental Psychology should be addressed to

Professor Samuel W. Fernberger, Editor JOURNAL OF EXPERIMENTAL PSYCHOLOGY,
University of Pennsylvania, Philadelphia, Pa.

Contributions intended for the Psychological Monographs should be addressed to

Professor John F. Dashiell, Editor PSYCHOLOGICAL MONOGRAPHS,
University of North Carolina, Chapel Hill, N. C.

Reviews of books and articles intended for the Psychological Bulletin, announcements and notes of current interest, and *books offered for review* should be sent to

Professor John A. McGeech, Editor PSYCHOLOGICAL BULLETIN,
Wesleyan University, Middletown, Conn.

All business communications should be addressed to

Psychological Review Company, Ohio State University, Columbus, Ohio

THE PSYCHOLOGICAL REVIEW

is indexed in the

International Index to Periodicals

to be found in most public and
college libraries

REMINISCENCE AND ROTE LEARNING

By LEWIS B. WARD
Wayne County Training School

In the first few minutes after the serial learning of nonsense syllables in the traditional Ebbinghaus manner, reminiscence is clearly demonstrated. Other investigators have not been able to observe reminiscence in nonsense material because they have waited too long to test retention. The theoretical implications of this discovery are discussed.

PSYCHOLOGICAL MONOGRAPHS Whole Number 220
64 pages \$1.00

REASONING, REGRESSION, AND COMMUNICATION IN SCHIZOPHRENICS

By NORMAN CAMERON
Henry Phipps Psychiatric Clinic

The reasoning process of twenty-five schizophrenics was studied by the method Piaget has developed and applied to children. A comparison of the present findings with Piaget's indicates that regression to infantile methods of reasoning does not characterize the schizophrenic. The volume will be of value to those interested in problem solving in normal adults as well as to those who are primarily concerned with the abnormal.

PSYCHOLOGICAL MONOGRAPHS Whole Number 221
34 pages \$.50

THE PSYCHOLOGICAL REVIEW COMPANY
THE OHIO STATE UNIVERSITY
COLUMBUS, OHIO

